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THE

GEOLOGICAL MAGAZINE.

NEW SERIES. DECADE IV. VOL. VI.

No. I.—JANUARY, 1899.

ORIGINAL ARTICLES.

I.—Fulgurites from Tupungato and the Summit of Aconcagua.

By Professor T. G. Bonney, D.Sc., LL.D., F.R.S., V.P.G.S.

THE members of Mr. E. A. FitzGerald's expedition to the Chilian Andes in 1896–7[1] made a rather large collection of rock specimens from the higher regions, which has been entrusted to me for examination. The descriptions will be published in Mr. FitzGerald's forthcoming book, but as two are exceptionally interesting I wish them to appear in the Geological Magazine. Permission for this has been kindly granted by him and by his publishers, Messrs. Methuen, to whom I return my cordial thanks.

The specimen of greatest interest is a block of andesite with remarkably fine fulgurites, taken from the actual summit of Tupungato (20,260 feet) by Mr. S. M. Vines, a member of the expedition, who made the first ascent in company with the noted Swiss guide Zurbriggen. The mountain-top is a curving plateau, from which three peaks rise up to a maximum height of about 300 feet. The crater has disappeared, but a little scoria was collected, from which I conjecture that some traces of it may still be found.

The first specimen bears this label: "Found loose in quantities on the top of highest of the three heaps or peaks at summit of Tupungato." It is a block rather irregular in shape, one surface being somewhat convex and the corresponding one concave, but the dimensions may be roughly given as $6 \times 6 \times 4$ inches; it is slightly weathered on the older surfaces (some being fresh fractures). The rock is compact, except for a few minute vesicles; in colour a dull purplish brown or madder, and spotted with small white felspars, the largest of which vary from about one-tenth to one-sixth of an inch in diameter. Under the microscope we find the minerals of early consolidation to be (*a*) iron-oxide; (*b*) brown hornblende, not seldom fairly idiomorphic, very variable in size, and sometimes

[1] For a summary of the work, see the Geographical Journal, vol. xii, p. 469.

including grains of felspar and of iron-oxide; (*c*) biotite, only in a very few ill-developed flakes; (*d*) one or two small grains of augite. The felspar, plagioclase, is variable in size; inclusions are inconspicuous; some of the larger grains are broken; these in extinction agree best with andesine, but possibly oligoclase may be also present; a small zircon is seen, and a few little vesicles. The base is a glass crowded with microliths, probably felspar, and tinted with ferrite. Thus the rock is a hornblende-andesite.

In this block fulgurites are abundant, commonly perforating it, but occasionally forming channels on the surface. The tubes not seldom branch irregularly and vary in size. When fairly circular in shape the diameter ranges from about one-fifth to one-third of an inch, but the latter measurement is exceeded at a fork. The irregularity of their course makes a precise statement impossible. Where the tube shows on a broken surface we see that it is coated at first with a film, hardly so thick as the finger-nail, of a reddish-white or warm-grey tint, over which is a layer of glass of a greenish colour, not unlike some of the lighter-coloured varieties of common bottle glass, and occasionally presenting in this respect and in fracture a superficial resemblance to some varieties of olivine. This glass appears not to form a perfectly regular coating, for sometimes it is almost absent, at others it occurs in patches, the thickness of which is about one-eighth of an inch; while at others the tubes are almost choked by it, but in the last case it forms an aggregate of separate clots rather than a solid mass; the interstices, in many instances at least, being due to bubbles. In a section of one branching tube the aggregated glass extends for about two inches inwards from the surface and for about one inch in a transverse direction at a short thick offshoot.

On careful examination of the material I came to the conclusion that a thin slice could not be made, and that the attempt to obtain it might seriously damage the specimen, so that I have studied the glass by crushing some small fragments and examining the powder under the microscope. The fragments vary in form, but flattish chips are not uncommon, and occasionally they have a rather fibrous structure; curved surfaces are visible, probably indicating the former presence of bubbles. The glass now appears to be of a very pale-yellow tint; clear, and containing but few enclosures. These are either minute bubbles or very small flaky microliths of ferrite and opacite.

In the fulgurites on a specimen of a very similar rock (measuring about $4 \times 3 \times 2$ inches), which was "lying loose in quantities on the summit," the tubes run slightly smaller than in the last specimen; the glass is similar in appearance, but just a shade darker in colour. With a strong lens many minute vesicles, or their broken surfaces, can be seen. Under the microscope the glass is almost the same as in the other case, except that it seems slightly more strongly coloured; bubbles and definite flakes are perhaps rather less numerous, but here and there are tiny brown patches. The white film mentioned above occurs in specimens from both; it appears to

be a layer of imperfectly fused rock, the transition from this to the perfect glass being very rapid. It is more or less opaque, but signs of felspar microliths can be detected. Evidently the rock was not materially affected by the lightning for more than about a hundredth of an inch beyond the fused part.[1]

The next specimen is from the actual summit of Aconcagua, which was also collected by Mr. Vines. In the rock itself there is nothing exceptional, but it is interesting as being from the highest point yet certainly reached. This, according to a trigonometrical measurement by Dr. Güssfeldt, is 22,867 feet above the sea.[2] The summit is a square plateau, 75 paces each way, sloping slightly to the north. But few specimens were brought from the uppermost part of the mountain, none of which are scoria. This rock is no doubt from a dyke or a fragment of a flow, so that the crater has probably disappeared even more completely than from Tupungato. Photographs show that Aconcagua has been greatly sculptured by meteoric agencies; at least as much, though on a grander scale, as the Puy de Sancy in Central France. The specimen bears this label: "Loose fragment from the summit of Aconcagua; all the neighbouring rock of the plateau, about 75 yards square, appears to be the same." It is a rather triangular piece of rock, measuring from 5 to 6 inches along the side and about $2\frac{1}{2}$ inches in the thickest part. Its colour is a warm purplish-grey; the old surfaces are rather speckled and have weathered to a reddish or yellowish brown tint; small whitish felspars and a black pyroxenic mineral, with possibly some iron-oxide, can just be detected by the unaided eye, and a few vesicles are present. These larger minerals prove on microscopic examination to be: (*a*) Felspars rather variable in size, often about one-fiftieth of an inch long, much as described above, probably andesine. (*b*) Hornblendes, fairly regular in outline, but occasionally with blunted angles or a corrosion border, sometimes even rounded; pleochroic, changing from pale greenish-brown to a deep sienna-brown; in one or two cases they form a kind of skeleton, owing to the inclusion of small irregularly-shaped felspar crystals. (*c*) A few grains or crystals of augite of a pale-brown tint inclining to green. (*d*) Iron-oxide. The base in which these crystalline grains are embedded is sprinkled with opacite and clouded with ferrite, being apparently a slightly decomposed glass; it is crowded with microliths of plagioclase, which give rather small extinction angles. Hence the summit rock of Aconcagua is a hornblende-andesite. This determination is fully confirmed by a chemical analysis, which has been made for me in the Chemical Laboratory of University

[1] Fulgurites from Mont Blanc and from Monte Viso (collected by Mr. J. Eccles) have been described and figured by Mr. F. Rutley (Quart. Journ. Geol. Soc., vol. xli, p. 152, and vol. xlv, p. 60); also (in a serpentine) by Miss E. Aston and the present writer (id., vol. lii, p. 452).

[2] Two peaks of greater elevation in the Himalayas are said to have been climbed, but in each case much doubt exists as to the accuracy of the identification. Pending the determination of the heights lately reached in the Bolivian Andes by Sir M. Conway, and perhaps in any case, Aconcagua is the highest peak which has been certainly scaled.

College (London) by R. W. Gray, Esq., to whom I return my sincere thanks. The following is the composition :—

SiO_2	60·32
P_2O_5	0·05
Al_2O_3	17·10
Fe_2O_3	4·74
FeO	1·12
MnO	trace
CaO	3·51
MgO	2·89
K_2O	2·11
Na_2O	5·06
H_2O	1·99
Loss on heating to 100 C.	0·81
	99·70
Specific gravity	2·609

II.—The Scandinavian Ice-sheet and the Baltic Glacier: A Sceptical Commentary.

By Sir Henry H. Howorth, K.C.I.E., M.P., F.R.S., F.G.S.

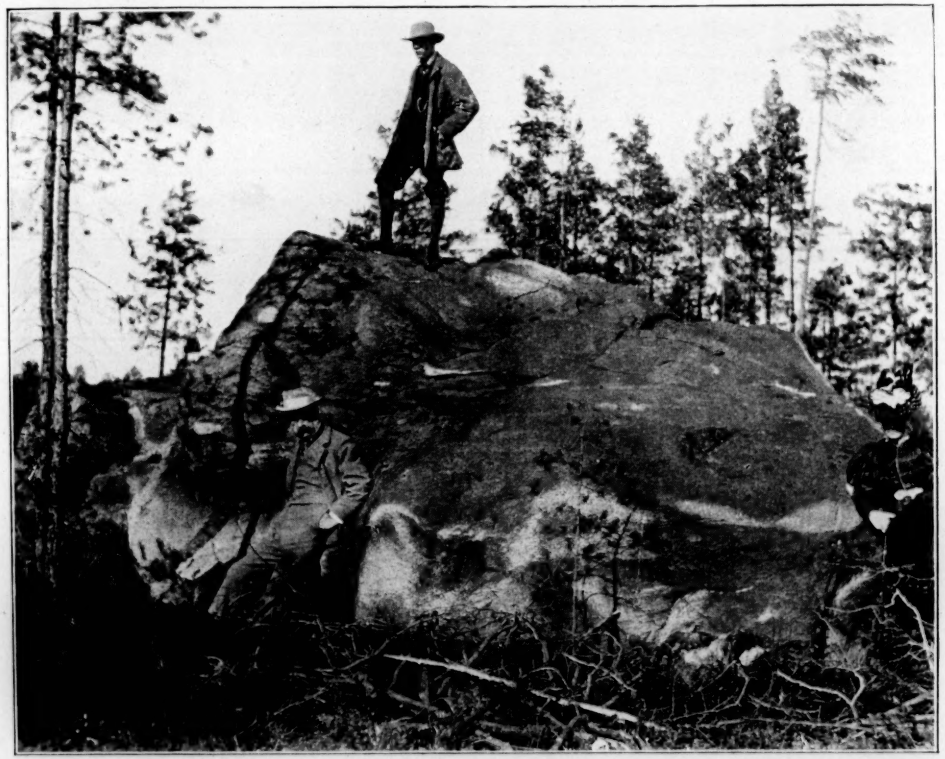
(PLATE I.)

IF the paper on the recent geology of Sweden which has already appeared in the Geological Magazine[1] is sound in argument (and I have not met anyone yet who has answered it), it follows that the views ordinarily current in regard to the glacial geology of Northern Europe will have to be greatly modified. Scandinavia is confessedly the great focus and centre of the phenomena which have been interpreted as glacial. From Scandinavia the most interesting and widely travelled of the erratics which are found in other parts of Europe have come, and if the drift of Northern and Central Europe were distributed by an ice-sheet, then it is universally conceded that that ice-sheet must have culminated in Scandinavia and moved thence in various directions; but it seems to me that the analysis which I have attempted to give of the actual phenomena makes it difficult to attribute the most important of them to ice action at all.

It is perfectly plain, as every native geologist admits, and as is admitted by every geologist of note who has visited the country, that Scandinavia has been submerged in quite recent times to the extent of at least 600 feet. The terraces, the raised beaches, the shell deposits, the stratified sands and gravels, etc., all unmistakably point to this conclusion as inevitable.

If so, it is also inevitable that if there has been a Glacial Period in Scandinavia at all it was before and not after the submergence. The native geologists are at one about this conclusion also. We cannot conceive these raised beaches and shell deposits, and these widespread beds of stratified sands and gravels, surviving the pounding and crushing and thrusting of a vast ice-sheet, which is

[1] See Geol. Mag., 1898, May (pp. 195–206) and June (pp. 257–266).



Very large rolled and split Crystalline Boulder, from the top of the Mountain above Falun, Sweden.

supposed to have carried erratics to the Carpathians on the one hand and to Britain on the other. They would all have been swept away or their stratification destroyed. So would the vast and mysterious åsar, the kames, etc., of whose aqueous origin it seems impossible to doubt.

Hence the Swedish geologists have, without exception, put their great submersion after their Glacial Period, just as their submergence is put by the American geologists after theirs.

Let us now see what follows from this. In the first place, as I argued in a previous paper, following in the wake of Pettersen, the smoothed and polished faces of rock and the hummocky islands on the coasts of Scandinavia, which have been so very generally accepted as presenting the most palpable traces of ice action, are much more likely to have been the result of the wearing of the tide armed with gravel, or stones, or shore ice. Pettersen seems to me to have clearly shown that this was the case on the Norwegian coast, and I believe it to have been equally so on the Lofoden Islands. We cannot suppose that these rocky faces and surfaces should have been exposed, as they must have been, to submarine action and erosion for a long time (as is evidenced by the mollusca in the raised beaches) without having been smoothed and mammillated as they are. The string of islands which fringe the Norwegian coast, the similar islands in the Christiania Fiord, the Mälar, and the Åland archipelago, all seem to me to have one aspect, the same as the archipelago of islands north of America, which have recently emerged from the sea, and their aspect seems due to the like cause; and I cannot separate the inland smoothed rocks of Sweden situated in a region which has been unmistakably submerged from the same category.

If this be so, *a fortiori* must we attribute the striations and scratchings upon these same smoothed rocks to the handiwork of some force acting during or since the submergence. It is inconceivable that these scratches and striæ should have survived the wear and tear of a submergence in these tempestuous latitudes for a long period, and the grating and rubbing of shingle and gravel and shore ice. I cannot, in fact, see how the scratches and striæ on the walls of the fiords in Norway, which have been so frequently quoted as proofs of the handiwork of an ice-sheet, can have been the work of any ice-sheet that was active before the great submergence; and, as we have seen, it is only before the submergence that we have any warrant for believing in such an ice-sheet at all. It is not only that the sea must have worn away all such minute marks, but we actually have evidence that it cut back the rocks, forming a series of recessed terraces called *strandlinier*. How could glacial striæ survive a cutting back like this?

In regard to the long striæ which traverse the Dovrefelds for many miles, and often run athwart their line of drainage, it seems impossible to understand how, if the ice culminated on the fells and moved outwardly in various directions, it could have cut and scored these straight long striæ and incisions. These striæ, running in straight lines right across the watershed of the country,

cannot be separated from the aberrant course followed by many of the Swedish boulders, and described, *inter alios*, by Professor James Geikie in the following paragraph. He says: "Dr. Törnebohm and Professor Högborn have shown that the glacier-carried erratics of Jemtland demonstrate that the ice has passed from east to west—that is, right against the slope of the land; and, according to Keilhan, similar blocks which could only have come from Sweden are now found in Trondhjems Fiord; while Pettersen has recorded similar facts in connection with the glacial phenomena of Northern Norway. The most remarkable circumstance in connection with some of these blocks consists in the fact that they occur at a considerably greater height than the rock from which they have been derived. Thus, at Areskutan, Törnebohm found blocks at a height of 4,500 feet which could not possibly have come from any place higher than 1,800 feet." ("Great Ice Age," 3rd edition, p. 424.)

I cannot understand the process by which these striæ are supposed to have been made, and these blocks to have been moved, by a great ice-sheet culminating, not on the mountains, but in the lowlands far away to the east of the mountains, and moving, not along the lines of easiest descent and of least resistance, but marching right across the high range of mountains. It seems to me as likely a process as that the Rhône glacier should have climbed up Mont Blanc in preference to flowing down the Rhône valley; and the phenomena in question seem to me to be only consistent with a great and serious alteration in the surface contour of the country, accompanied by some widespread diluvial movement either after or coincident with the making of the striæ and dispersion of the boulders.

Let us now turn to other characteristics of the boulders and erratics, which are such a prominent feature in the surface beds of Sweden and Norway. The Northern Glacial geologists differ from our own in one important particular, namely, in discriminating between the different materials which form their surface beds. They would be surprised if they came to England and saw how it is the fashion here to describe as glacial, beds consisting entirely of rolled gravels or boulders, often intercalated with pockets and layers of stratified sands and false-bedded brickearths: beds quite different to true moraine materials, which are heterogeneous, unsorted, and consist of a mixed mass of mud, sand, and stones, chiefly angular and unrolled.

In Sweden the rolled and rounded gravels and boulders are directly attributed to the action of water, not water running in so-called subglacial streams, which by our geologists are supposed capable of travelling to and fro everywhere, irrespective of the contour of the country, but to submarine submergence. I cannot understand how these millions of boulders of all sizes and materials should have been manufactured in the transcendental and largely imaginary boulder mills which are postulated as having underlaid the ice-sheet. A great proportion of them are composed of rocks which are not found rising and beetling above the general level of

the country, but which must have been deeply covered by the ice-sheet, if it ever existed; and as I am no believer in what the Americans call the plucking theory, or the tooth-drawing theory as Professor Bonney calls what he so ably denounces, I consider this a fatal objection to the subglacial formation of such boulders. Nor can the enormous masses of rock, as large as small houses, whose rounded angles clearly testify to their having been rolled, be conceived as rolled by any subglacial streams on this side of Saturn! These rolled boulders seem to me to testify most completely to the handiwork of the sea during a widespread submergence, a submergence which, as we have seen, is postulated by every inquirer into Scandinavian geology, native and foreign, who has explored the country. I take it, in fact, that these rolled boulders and pebbles were made by the same force and at the same time as the rock surfaces of Scandinavia were polished and mammillated.

The Swedish geologists do not treat these rolled boulders, and the beds in which they chiefly occur, as glacial, but as water-worn and arranged by water, and notably is this the case in the great deposits of them found in the åsar, eskers, etc.

What the Swedes treat as moraine-stuff is the accumulation in certain places of masses of angular débris, mixed with sand or loam, which occur in certain limited areas and which have the internal structure of moraines.

It is a curious and eloquent fact that whereas these two sets of deposits generally occur separately, they also sometimes occur mixed, and they occur mixed in deposits like the åsar, which seem unmistakably to be of aqueous origin. Whatever the ultimate origin of these accumulations of angular or subangular materials, they would appear, therefore, to have existed when the submergence was still in operation. If, however, they had existed throughout the period of submergence, how is it they have not been sorted into different materials, according to their specific gravity, and how is it the stones have not been rolled into boulders and had their edges rounded? I cannot see how the Swedes get over this dilemma. They clearly put their Glacial Period, to which they attribute these so-called moraine heaps and mounds, before their great submergence. How, then, did these materials escape being water-worn?

According to the view I have maintained, these angular stones and blocks, which occur in such vast numbers in certain districts, like Dalecarlia, are the direct result of the breaking of the strata into fragments, when the district of Central Sweden was violently upheaved, and the waters which then covered a large part of it and of Finland were scattered and driven in all directions. In such a case there would be no rounding or rubbing of the angular débris. The same waters which piled up the rounded boulders into mounds and heaps would pile these up also, and in cases where the two were in proximity to one another they were inevitably mixed.

Let us now turn to another point, namely, the traction and movement of the very large blocks. This is a subject of great difficulty

to most geologists, and makes many of them accept the extreme glacial view. They fancy that ice, and ice only, is the possible motive engine of the stones. The fallacy of this argument is well tested in Scandinavia.

The one thing which perhaps has struck me more than any other, in travelling through those parts of Central Sweden where the great stones occur most frequently, is the fact that we cannot separate them from the beds of stratified sand, the åsar, etc., which occur there, and are so clearly of aqueous origin. This provenance of many of the boulders is not disputed. Thus Professor Geikie says: "Erratic blocks are frequently found perched upon the top of an ås, or plentifully sprinkled along its sides, and sometimes also they occur in the interior, especially towards the top or the base." Again, he says: "Frequently the shelly clays exhibit disturbed and contorted bedding, and they often contain erratics." And further: "The beds next in succession consist of marl clay, sand, and gravel. They contain numerous shells of Baltic species, and large erratics often rest upon them."

I have myself verified this fact in various parts of Sweden, and notably in the case of the åsar and mounds north of Upsala, whose backs are strewn with great boulders. How can we account for the moving of these great stones by ice? The postulated ice-sheet, according to the Swedish geologists and those who have written on the subject from personal knowledge of the country, had passed away long before these stratified sands and åsar were laid down. It is impossible, therefore, to attribute the planting of the stones on their very backs to the operations of an ice-sheet. If they were not carried and deposited by the ice-sheet it is still more difficult to understand how they could have been moved by icebergs or ice-rafts. Icebergs are only the calves of glaciers. The only stones they carry are the stones carried by the glaciers. Now, glaciers do not have great masses of stone of the size of huts frozen into their feet, and if they had they could not roll them along so as to rub off their angles as these great blocks are rubbed. If they had been carried on the glaciers' backs they would be quite unweathered and angular. Apart from this, the stones themselves come from districts which could not at the time of the great submergence, when these supposed icebergs were floating about, have been exposed to the air, but must have been deeply covered with water. The water must, in order to deposit these stones, have covered the tops of the åsars themselves many fathoms deep. If this were so, how could icebergs or ice-rafts pick up the stones at all? People who talk of icebergs and ice-rafts in this fashion should visit the country and see where the beds lie *in sitû* in Dalecarlia, whence the big stones have mainly come, and they will realize how impossible and fantastic the notion is, that either an ice-sheet, or icebergs, or ice-rafts could have moved them from their original homes to where they are now found in Sweden. Ice in any form, therefore, seems quite a transcendental cause to appeal to for the transport and deposition of these blocks, and if we put ice aside, then, as I have said over and over again,

there is only one force which seems competent to account for the portage in question, and which also accounts for the other correlated phenomena, and that is water on a great scale moving rapidly—such a mass of water as would be sped along if North Central Sweden were to be rapidly upheaved and discharge in all directions the waters that once covered it.

Another word with regard to the distribution of these stones. If we want a good test of whether a large mass of scattered angular boulders or erratics were distributed by land-ice or water, we shall find it in this fact, that whereas water in moving along sorts out the stones it carries, according to their weight and size, dropping the big ones first and the lesser ones afterwards, a mass of moving ice like a glacier does not sort them at all; it carries on its broad back a stone as big as a house just as easily and just as far as it carries a pebble. This can be tested in any Alpine valley. Now, if we apply this test to Scandinavia, we must conclude that the stones were carried by water; a few very big ones were no doubt carried a long way, as they would naturally be by a vast torrential flood, but the great mass of them are arranged roughly according to their specific gravity, getting smaller as we go farther away from their place of origin.

Let us now turn to another feature of the so-called glacial deposits of Sweden which seems to me quite inconsistent with the so-called ice-sheet theory. This is the distribution of the marls and limey clays, of which a beautiful map has been published by the Swedish surveyors. If we examine them, and also note the blocks of Baltic limestone which occur in Sweden, we shall find that we establish at least two centres of distribution: one from the Silurian limestones of Gothland, notably from the district of which Kinnekulle is the focus, and the other in the district north-east of the Mälar lake, from which the limestone has apparently been recently and violently stripped, leaving only a small insular piece near Gefle, but of which the remains both in the form of powdered limestone and of blocks and boulders occur over a wide area.

What I am urging is, that anyone who will examine the two areas just named, where the Silurian débris occurs, will realize how desperately difficult or impossible it is to account for this particular phenomenon by ice action of any kind, and how unmistakably it points to a great breakage of the surface and the distribution of the débris by water.

It seems to me that whenever we apply a really critical analysis to the separate facts and factors of the problem as presented by the surface of Scandinavia, we are met by the same stupendous difficulties in attributing them to ice action. Nor would the idea have dominated men as it has if they had not for so long done homage to the Baal of a spurious Uniformity and the notion that Nature never acts in a violent and spasmodic way. The great argument of all, however, remains, and I have pressed it in season and out of season. It is not a geological one but a physical one, not based on fantastic speculation but upon precise mechanical laws,

and that is the impossibility of understanding how any ice-sheet could have existed in Scandinavia of such portentous depth and potency as to force débris thence to the Carpathians on the one hand and to Holland and Britain on the other.

All the evidence I know points to Scandinavia being higher now than it was in so-called Glacial times, and points to its having risen at least 600 feet since the drift was distributed, and we have no evidence that I know of that it was ever higher than it is now. As it is, the elevation of the Dovrefelds is so moderate that it is impossible to understand their nursing a great ice-sheet. In fact, we know that such an ice-sheet never covered their higher parts or those of the Lofodens, for they are rugged and not smoothed by ice. Pettersen has proved as clearly as can be that whatever ice was upon them in so-called Glacial times could not drive the stones even as far as the string of islands down the Norwegian coast. It is incredible, therefore, to suppose that any ice they supported could have driven the stones right away to Smolensk or to Brunswick. The contention that it did so, which is repeated, parrot-like, by one writer after another, seems to me, in the absence of some evidence of its physical possibility, completely childish.

But, as I have before remarked, the majority of the stones, and notably those found in Britain, did not even come from the Dovrefelds or the higher parts of Scandinavia, but from the lowlands on the borders of Viken and the undulating plains of Central Sweden. *A fortiori* it is impossible to understand how any ice-sheet could have taken up the stones there and carried them to where they are found.

The arguments I have used in this and the preceding papers have been entirely or almost entirely geological. There remains, however, another, which I will only shortly refer to, namely, that based on the biological evidence.

According to the glacialist, the whole of Scandinavia was covered with ice for a long period, and this ice, radiating from that peninsula, extended for hundreds of miles south of the Baltic. During this period we are to believe that the whole fauna and flora of this immense region was completely extirpated, or rather migrated, goodness knows how and goodness knows whither. When the ice had disappeared it is supposed to have gone back again unchanged and unaltered. This monstrous paradox has been widely believed in by geologists, and been passed on by them to the zoologists and the botanists.

I tried to show in a former paper how this theory, which had also been applied to Greenland, was quite at issue with the evidence available from that much more northern land, and I have no doubt myself that in Scandinavia also the theory of the extermination of all plants and animals during the Drift period and their reintroduction afterwards is just as fantastic. I do not know of a tittle of evidence in favour of the view here condemned except and beyond the mere fact that, if the glacial theory as usually taught be sound, it necessarily involves adding this most tremendous hypothesis

to it and to our geological creed. To those who, like myself (if there be any), who utterly repudiate the glacial theory, there is no such necessity. I do not believe that the animals and plants of Scandinavia were entirely driven out and sought shelter in some unknown region, perhaps in Jermyn Street or the University of Edinburgh, and that they returned again in all their fulness and completeness, without variation and change, to reoccupy their old country.

I believe, on the contrary, that the animals and plants of Scandinavia are directly descended from their ancestors, who lived in the same country before the distribution of the drift, and that they have lived an unbroken life there since. A few species were doubtless destroyed by the revolution which distributed the drift; a few have probably disappeared from the natural decay that goes on everywhere among species as among individuals. The uplifting of Scandinavia to 600 feet or more may have caused the disappearance of others, or their migration to lower levels, but in the main and substantially there has been no break in the continuous animal and vegetable life in Scandinavia. The diluvian movement in which I believe, and whose handiwork I have tried to describe, doubtless caused temporary desolation in those districts over which it passed, but it did not pass everywhere, and those localities which were depopulated temporarily were presently occupied by colonists from other localities which had escaped the scourge. This is my view, and assuredly if science is based upon the discovery of rational and adequate explanations of the economy of nature, the biological argument here used must weigh a great deal in deciding the problem. I only use it, however, as a subsidiary argument.

Let us now consider a subordinate problem with which the one I have discussed in this and the previous paper is closely bound up, namely, the so-called Baltic Glacier. The Baltic is a great stumbling-block to the extreme glacial men. This vast trough intervening between Scandinavia and Germany and Russia, offers a stupendous difficulty to any glacial geologist (if there be one) who takes notice of ordinary mechanical and physical laws.

The Scandinavian débris which is found in North Germany as far as the Carpathians is supposed to have been carried thither by a vast ice-sheet, which first traversed the Baltic right athwart its length. Neglecting all the usual methods adopted by moving liquids and viscous substances, having found itself in this trough it did not discharge itself by following the path of least resistance but climbed the steep bank on the further side. How this is to be explained I confess I do not know, and those who are responsible for the postulate have never tried to explain it; but this is not all. The boulders which this ice-sheet is supposed to have carried to Central Germany and to Britain are not angular boulders, such as glaciers carry on their back, but are for the most part rounded or subangular, and according to the Glacial champions were moved along by the ice-sheet under itself in the form of what is called a *moraine profonde* (a thing which is quite incomprehensible to me).

Now, granting the possibility of a *moraine profonde*, as understood and postulated by the Glacial champions, how are we to explain this action of the ice? Ice is not a rigid substance but a viscous one; when a viscous body, in moving along, meets with a deep depression, it does not move down into the hollow and up again on the other side, but it fills up the depression.

Now, granting that a great ice-sheet came down from Sweden towards Germany, dragging along with it and underneath it the postulated *moraine profonde*, when it reached the Baltic trough it would pour down into it and fill it just as water fills a hole in a river bottom, and having filled the hollow it would become embayed in it. The ice coming on behind would then flow over the embayed ice, which would remain as quiescent and unmoved as I believe the water in deep pits in a river bed is; but what about the *moraine profonde* (that unverified postulate of the wilder glacialists)? What would become of it? If, as we are told, an ice-sheet can drag a *moraine profonde*, consisting of loose materials many scores of yards thick, along with it over a rocky bottom, are we further to believe that it could do so over another ice-sheet as long as the Baltic is wide, and thus carry its stony load safely into Germany and Russia? Why, Mr. Maskelyne and all his confrères are nothing to these wonderful wizards, who are responsible for teaching a noble science to the ingenuous youth of the closing years of this century of marvels.

The glacialist apparently argues, however, that the ice would not be embayed in this fashion, but would move on without any difficulty down and up again and right across the great depression, being impelled by the force behind it in some unexplained way, and would carry its *moraine profonde* along with it. This presents another difficulty: if the ice were not embayed, the stones surely would be; they would be caught in the hollow and accumulate in it until that hollow was filled up level with the top of the bank on either side. But the Baltic is not full of stones; it is full of water: hence the dilemma is twofold.

I am bound to be quite candid and to say that a similar difficulty applies to the aqueous distribution of the Scandinavian boulders over the plains of Germany. Water rolling along loaded with stones would inevitably drop its burden into the first ditch it rushed athwart and fill it up, and the Baltic for our present purpose is only a big ditch. How, then, comes it to be filled with water instead of being choked up with drift?

This difficulty compels those among us who do not approve of appealing to causes until we have tested their competency, to try to offer some explanation. The explanation I would offer is, that the history of the Baltic depression has been very generally misunderstood. Instead of being in its present form an old feature in physical geography, one part of it is in fact a recent one, namely, that part which runs east and west to the south of Sweden and to the north of West Prussia and Mecklenburg. I have been satisfied for some time, and I find that the view has been urged in Germany

by at least one geologist of repute, that the east-and-west section of the Baltic, together with the Sounds between the Danish islands, are in fact very recent features geologically speaking. It is very probable that when the Drift was distributed they did not exist at all, at all events in the place where we now find them, but that a continuous land surface connected Scania and Denmark with Mecklenburg and Pomerania.

I hope to discuss this issue and the consequences dependent on it on a later occasion. The present paper concludes what I have to say in criticism of the Glacial theory as applied to Norway and Sweden, and until some one tries to answer what I have urged I shall conclude that it is unanswerable, which is not an unsound position for a heretic to take.

III.—The Origin of Mammals.[1]

By Professor O. C. Marsh, M.A., Ph.D., LL.D., F.G.S.;

THE question as to the origin of mammals is not new, but one of a series of similar nature and difficulty. The origin of Birds, of Reptiles, of Amphibians, and of Fishes really precede it, and offer less difficulties in solution. The answer to each, in my opinion, belongs to the future, and how far it may profitably be sought in the present limited state of our knowledge is a fair question in itself.

Too often in the past, a discussion on the origin of Mammals has seemed a little like the long philosophico-theological controversies in the Middle Ages about the exact position of the soul in the human body. No conclusion was reached, because, for one reason, there were no facts in the case that could settle the question, while the methods of investigation were not adapted to ensure a satisfactory answer. This discussion has been on a much higher plane, and we have had an admirable presentation of one side of the case. I cannot, however, quite agree as to the value of the facts and theories that have been presented, and shall consider the question from another point of view.

The Mammals, as we know them to-day, are classed by themselves, yet contain such diverse groups that it may fairly be regarded an open question whether all have a common origin. The attempt to ascertain whence they came is likely to bring out indications that they may have had several sources of origin, and this, if so, may help to explain the great diversity among them.

It is of course evident that some of the most characteristic features of recent mammals—for example, the hairy covering, the circulatory system, and the milk glands—cannot be used in a comparison with fossil forms. The osseous structure only is now available in the early mammals and other vertebrates, and in this alone points of resemblance must be found if different groups are connected genetically.

[1] Remarks in the general discussion on the Origin of Mammals at the International Congress of Zoology, Cambridge, England, August 25, 1898.

In considering the relations of reptiles to mammals so far as the fossil forms are concerned, which seems to be the main question now before us, I have only time to speak of the skull, and shall refer to some of its salient features already mentioned in this discussion; namely, the teeth, the squamosal bone, the quadrate, the occipital condyles, and with them the lower jaw. These perhaps may serve as crucial points in distinguishing the skull of a reptile from that of a mammal, even if they fail to indicate a near affinity between them.

The different kinds of teeth seen in the reptiles regarded as mammalian in type I consider of comparatively small importance, for the reason that the same general forms of teeth are to be found, not merely in the reptiles supposed to be nearest to mammals, but also in other groups widely different. In the Crocodiles, for example, the extinct genus *Notosuchus*, recently discovered in Patagonia, has all three kinds of teeth well distinguished. Again, some of the Dinosaurs, especially the genus *Triceratops*, have teeth with two roots, a supposed mammalian character. In some Fishes, also (*Anarrhichas*), three kinds of teeth may be seen. It is more than probable, therefore, that the peculiar resemblance between the teeth of mammals and those of the lower vertebrates is merely one of parallel development, the adaptation being along similar lines, and in no sense an indication of genetic affinity.

The great development of the squamosal bone in Theriodont reptiles is not alone, for somewhat similar proportions are found in some other reptiles,—for example, in the Plesiosaurs, where the squamosal is very large, and wrapped round the quadrate. In some of the Dinosaurs, also (*Torosaurus*), the squamosal has an enormous development, while the quadrate remains of very moderate size.

The quadrate bone, always present in Birds, Reptiles, and the other lower vertebrates, is well known as the suspensorium of the lower jaw, which meets it with a concave articular face. The quadrate, however, appears to be wanting in mammals, or at least has not yet been identified with certainty.

What represents the quadrate bone in mammals, is a vexed question in itself, and some of the best anatomists in the past, Cuvier, Owen, Peters, Huxley, and others, have endeavoured to solve the problem. The tympanic bone, the incus, and the malleus have each in turn been regarded as the remnant of the quadrate, but up to the present time the question has not been settled. It is not improbable that the quadrate may have coalesced with the squamosal.

The occipital condyles of mammals, as well known, are two in number and separated from each other. This is not the case with any true reptile, although the contrary has been asserted. The nearest approach appears to be where there is a single bifid condyle, cordate in shape, with the two lobes meeting below, as in some reptiles and a few birds, but not separate as in mammals and amphibians.

Finally, in all known mammals, recent and extinct, the lower jaw is composed of a single piece, and has a convex condyle meeting the

skull by a distinct articulation. All reptiles, even those supposed to be nearest the mammals, have the lower jaw composed of several pieces, and these show distinct sutures between them, a profound difference that must be explained away before an approach can be made between the two classes.

It may fairly be said that the separate elements of the lower jaw, if present, would naturally be looked for in the Mesozoic mammals, and this point I have long had in mind. I may safely say that I have seen nearly every species of Mesozoic mammal hitherto described, and have searched for evidence on this point without success. I have also sought for the separate elements in the young of recent forms, but without finding any indications of them.

Besides the crucial points I have mentioned in the skull, there are others of equal importance in the skeleton, which I must not take time to discuss, but will venture to allude to one of them in passing. I refer to the ankle joint, which, when present, is at the end of the tibia in mammals, and in reptiles between the first and second series of tarsals. When we really find an approach between these two classes, the ankle joint will probably show evidence of it.

Having thus shown, as I believe, that we cannot with our present knowledge expect to find the origin of mammals among the known extinct reptiles, and that in attempting this we are probably off the true line of descent, it remains to indicate another direction in which the quest seems more promising.

Since 1876, when Huxley visited me at New Haven, and we discussed the probable origin of both Birds and Mammals, I have been greatly impressed by his suggestion that the mammals were derived from ancestors with two occipital condyles, and these were doubtless primitive Amphibians. I have since sought diligently for the ancestors of birds among the early reptiles, with, I trust, some measure of success, but this is a simple problem compared with the origin of mammals which we have before us to-day.

In various interviews with Francis Balfour in 1881, at the York Meeting of the British Association, we discussed the same questions, and agreed that the solution could best be reached by the aid of Embryology and Palæontology combined. He offered to take up the young stages of recent forms, and I agreed to study the fossils for other evidence. His untimely death, which occurred soon afterwards, prevented this promised investigation, and natural science still suffers from his loss. Had Balfour lived, he might have given us to-day the solution of the great question before us, and the present discussion would have been unnecessary.

The Birds, like the mammals, have developed certain characters higher than those of reptiles, and thus they seem to approach each other. I doubt, however, if the two classes are connected genetically, unless in a very remote way.

Reptiles, although much lower in rank than birds, resemble mammals in various ways, but this may be only an adaptive likeness. Both of these classes may be made up of complex groups only distantly related. Having both developed along similar lines,

they exhibit various points of resemblance that may easily be mistaken for indications of real affinity.

In the Amphibians, especially in the oldest forms, there are hints of a true relationship with both Reptiles and Mammals. It seems to me, therefore, that in some of the minute primitive forms, as old as the Devonian, if not still more ancient, we may yet find the key to the great mystery of the Origin of Mammals.

IV.—Prof. Hull's "Submerged Platform of Western Europe."

By Prof. J. W. Spencer, M.A., Ph.D., B.A.Sc., F.G.S.

MR. JUKES-BROWNE'S contribution to this subject, in the September number of the Geological Magazine, brought out a clear reply from Professor Hull in the next issue. Here the argument practically ceased, for in his letter in November Mr. Jukes-Browne turned his correspondence into an academic disputation of the meaning of the word 'escarpment,' for which other expressions could have been used, and the flat denial of the evidence submitted by Professor Hull. This is to be greatly regretted, for Mr. Jukes-Browne does not show evidence to the contrary, nor that he has investigated any of the facts for himself—such as the contouring of the fullest charts with isobars of two hundred or three hundred feet apart—before pronouncing his *ipse dixit*.

Mr. Jukes-Browne challenges the analogy drawn by Professor Hull between the drowned declivities, facing the continental shelf, and the slopes—whether steep, abrupt, or precipitous—which mark the descent from an elevated tableland, plateau, or even lower plain to the country below. Although Professor Hull uses the word 'escarpment' as covering this topographical feature, in exactly the sense commonly understood in America, I purposely avoid its adoption, so as not to allow of the diversion from the phenomena themselves. Mr. Jukes-Browne asks if Professor Hull can point out such a conspicuous declivity from one platform to lower plains "anywhere in Europe which has a length of 700 miles and a height above its base of 7,000 to 8,000 feet." The question appears to imply a negative answer, to be used as a refutation of Professor Hull's inferences. The question should not have been limited to evidence obtainable in Europe, where presumably such a feature is not likely to be found, but should have been extended to any region. With this latitude for reply, the reader may have the opportunity of knowing (as I certainly think Mr. Jukes-Browne has had) that the great face of the Mexican tableland is characterized by an abrupt slope reaching down 6,500 feet to the inner edge of the coastal plain, or over 8,000 feet to the Gulf of Mexico. This slope has a length of several times 700 miles, and extends from Mexico into the United States. In point of magnitude and abruptness, and also of the character of the dissecting valleys, this Mexican slope can quite as well be taken for comparison with Professor Hull's declivities as if found in Europe.

Irrespective of the origin of the tablelands, whether the result of stupendous faultings or of more gentle bulgings of the earth's crust, with the frontal margins fashioned by waves, or by both, or by other causes, the only point for consideration is the fact that the surfaces of these slopes have been moulded by the action of rains and streams, so as to be dissected by great deep valleys crossing all the formations, since they were last elevated above the sea. Besides many large valleys heading abruptly in the tableland, there are numerous short tributaries, called in America by the name of 'amphitheatres,' from two to ten miles long and thousands of feet deep. These begin abruptly in the edge of the high plateaux, and may be likened to gigantic wash-outs, such as are formed by the action of rains upon the soft materials of an elevated coast, and elsewhere. The elevation of the tablelands appears to have been interrupted by pauses allowing for the formation of base-levels of erosion or platforms or steps along the face of the great slopes and in the valleys themselves. Such features upon the edge of the high plateaux show the subaerial sculpturing of the great declivities regardless of the primary origin of the slopes. Then comes the important point that they are an exact representation, upon the surface of the land, of the submarine valleys and amphitheatres dissecting the slopes which descend from the submarine plateaux to the ocean floors. If the form and gradients of the sculptured features of the great terrestrial slopes, whether above or below the sea-level, be carefully studied, they will answer for themselves Mr. Jukes-Browne's remark "how the existence of river-made valleys can possibly prove the declivity to have been made by subaerial agencies passes my [Mr. Jukes-Browne's] comprehension"; for Professor Hull is only considering the formation of such surface features, when found beneath the sea, as evidence of changes of level subsequent to the subaerial sculpturing of the platforms and declivities themselves.

Nor is it alone on the margins of the plateaux overlooking the Gulf of Mexico that perfect topographic features similar to the drowned valleys are to be found. The Grand Cañon of the Colorado is over 200 miles long, and reaches to a breadth of thirteen miles and a depth of over 6,000 feet. It dissects a plateau having a height of over 8,000 feet above the sea. There is no lack of examples confirming the complete resemblances between the erosion features of the bold reliefs of the land and those beneath the surface of the oceans. The problem is one of great magnitude, and the amount of the material available is not small; but the conditions for investigations from the simpler to the more complex forms are more favourable in America than in Europe, and this is no doubt why geomorphic studies have not received the attention they deserve in Europe. The phenomena by which the geomorphist reads the history of the land features seem so simple that some geologists attempt to criticize the long and tedious investigations of physical geologists without having studied the grammar of the investigations. The result is precisely the same as would be the

determination of a marine horizon if one had never studied a fossil. Then the results are so remarkable that there is a sort of crime in presenting important facts which had escaped observation until such a recent date. Had Mr. Jukes-Browne carefully examined the data for himself, as Professor Hull has been doing—and it cannot be accomplished without devoting a great deal of time to it—I should be amazed to find him again writing, "I do deny (that the border" —i.e. of the submarine shelf—"can properly be called an escarpment or) that there is any proof of its having been fashioned by subaerial agencies." The parentheses are mine, as only the question of the sculpturing of the abyssal slopes is of any importance. Such a denial (without the accompanying evidence of actual investigation of the question), whether by an individual, or officially by the President, or even the whole council of any learned society, in the presence of the facts obtainable, can only cast reflection upon their discernment and displays a considerable degree of complaisance in denouncing the results of long and careful study in a new field of investigation, though living near that field, to which the critics are strangers. The investigator must have courage to continue in the face of denials by critics and councils, whose criticisms are usually the expressions of one man officially announced, and therefore of little more real value than the harm which they do. Moreover, human nature is the same among geologists as other men, and the announcement of newly interpreted phenomena is often not to be tolerated, except from the officials or critics themselves. It is the more surprising that Mr. Jukes-Browne should desire to deny the great subsidences of the continental margins, as he has been the advocate of the elevation, in the West Indies, from abysmal depths of the sea bottom, demanding a change of level of land and sea quite as great as Professor Hull now claims. His conclusion was based upon palæontological evidence, and beyond this field the objector certainly appears not to have grasped the researches of Professor Hull.

Considerations as to the point of time of the recent great elevations, as shown by Professor Hull, Mr. Jukes-Browne's denial of the elevation (in his last letter) would seem to render superfluous. But it may be added that the evidence in America, at least, indicates the late great changes of land and sea to have been in progress between the later Pliocene days and in the Pleistocene period, but that there is nothing showing the great changes (within that range of time) to have been actually synchronous; for in America, while the east was elevated the west was depressed, and again, while the west was lately being elevated, the east subsided; and so it may have been that the elevation of the two sides of the Atlantic were not simultaneous: but these possible conditions do not modify the more general hypothesis that some time in the early Pleistocene period the continents on both sides of the Atlantic were high.

NOTICES OF MEMOIRS.

I.—Notes on an Expedition to Christmas Island. By C. W. Andrews, B.A., B.Sc., F.G.S., Assistant in the British Museum (Natural History).

[Sir John Murray, having resolved to send a naturalist at his own expense to investigate the fauna and flora and the geology of Christmas Island, invited Mr. C. W. Andrews, of the Geological Department, British Museum (Natural History), to undertake the task. Having obtained the necessary leave of absence Mr. Andrews sailed from England on May 4th, 1897, and after an absence of 15 months he reached home on August 3rd, 1898. He made a considerable stay in Java on his outward passage; he also visited the Cocos-Keeling Islands on his return voyage. The expedition was most successful.

The collections made by Mr. Andrews during his stay on Christmas Island include geological specimens and many illustrating the botany and zoology. These comprise 274 mammals, birds, and reptiles; 183 mollusca; 1,419 insects; and 378 spiders, myriopods, crustacea, echinoderms, and worms. A selection of the specimens have been generously presented by Sir John Murray to the British Museum (Natural History).

The island is typically oceanic, and the mammals, land birds, and many of the insects found are peculiar to it; the geographical and geological observations made by Mr. Andrews are also of great interest.

The following abridged account communicated by the author is from a paper read by him before the Royal Geographical Society, November 28th, 1898.—H. W.]

One of the most interesting of the lonely islets of the Indian Ocean is Christmas Island, which lies about 190 miles south of Java, in lat. 10° 25′, long. 105° 42′. The seas around it are of enormous depth, and soundings of over 1,000 fathoms occur within two or three miles of its coasts. To the north and north-west is Macclear deep, in which 3,200 fathoms were found, and to the south is the more extensive Wharton deep, with upwards of 3,000 fathoms. The island, in fact, rises from the summit of the low submarine ridge which separates these two abysses, and on the westward end of which the Cocos-Keeling Islands are situated.

Seen from the south-west, the island appears as a long green ridge nearly level at the top, there being only slight elevations at the north-west and south-east ends. The ridge descends seaward in a succession of terraces, the upper ones bounded by comparatively gentle slopes, the lower by a high and nearly vertical cliff, below which there is a narrow platform sloping gently down to the sea-cliff. This is usually about 15 to 30 feet high, and is much undercut by the heavy swell that is continually breaking against its base. On approaching nearer it can be seen that the whole island is covered with a dense forest, only broken by the grey face

of the high inland cliff which runs round the greater part of the island, rising like a wall above the tall trees growing on the shore terrace.

If the coast be examined in a boat or from the top of the sea-cliff, it will be seen that a submarine terrace in the shape of a fringing reef is being formed round the greater part of the island. It varies greatly in width, and also in its depths below the surface; in some places it is partly dry at low-water, in others some fathoms deep. Outside the edge of this reef the water deepens suddenly.

The greatest length of the island, from North-East Point to Egeria Point, is about 12 miles. The greatest width from north to south is about 9 miles; the least 3½ miles. Its area may be roughly stated at 43 square miles.

The island consists of a central plateau, highest towards the north and east, and descending to the sea on all sides by a succession of terraces, separated by slopes or cliffs. In most places the arrangement of these, from the edge of the plateau downwards, is—(1) a steep slope strewn with blocks; (2) a broad terrace, followed by a similar slope (this seems to be wanting on the south); (3) a second terrace, terminating in a cliff 200 or 300 feet high; (4) the shore terrace, sloping gently down to the sea-cliff; (5) the present fringing reef. There are, however, many local differences, the more important of which will be noticed below.

The Central Plateau.

The edge of the central plateau is roughly parallel to the coast, receding farthest from it opposite the principal headlands. It is highest along its northern and eastern borders, where there is a raised rim, the average height of which above the sea is about 800 feet. Towards the south it slopes away so that its edge is only from 400 to 450 feet high, but there are some slight elevations above this general level. On the west the upper terraces are replaced by a gentle slope, and even the first inland cliff is not well marked except towards North-West and Egeria Points.

Along the raised rim of the plateau there are a number of hills, the highest of which (Murray Hill) occurs towards North-West Point. It is a nearly flat-topped hill, divided by an oblique valley into a larger and rather higher western portion and a lower eastern one; the greatest height is about 1,170 feet. The summit is formed by masses of dolomitic limestone, and on its lower slopes there are beds of shelly limestone, and a peculiar deposit which seems to be mainly made up of tiny spherules of altered volcanic glass. The outer face is very steep, but towards the south the land first descends gently, then rises a little, finally sinking to the general level of the plateau in a long gradual slope. On the south-western side there are occasionally patches of rounded pebbles, which are of a volcanic nature, and are, perhaps, derived from the bed above mentioned. Similar nodules occur in many places on the higher parts of the island.

The next highest hill (Ross Hill) is over South Point. It also has a flat top covered with dolomitic limestone, in which traces of gastropod shells are visible. On its outer side there is a low cliff, and below this a long steep slope covered with blocks of limestone in the wildest confusion, and thickly overgrown with creepers and brushwood. Towards the plateau also the descent is rather abrupt.

Over North-East Point is another elevation (Phosphate Hill), which, though not so high (900 feet), is particularly interesting on account of the extensive deposit of phosphate of lime which is found there. This substance is strewn over the surface in blocks of all sizes, and in some places it is found to a considerable depth; in others, however, it can be seen to rest directly on an irregular surface of dolomitic limestone, occasional pinnacles of which project through it. At the northern end of the hill the phosphate is found on both outer and inner slopes, but farther south on the plateau side only, the outer being occupied by a reef of limestone, which descends to the terrace beneath in a low cliff. The area actually covered by this thick deposit of phosphate of lime is about half a mile long by a quarter broad, but an immense quantity occurs in the form of irregular nodules and blocks scattered over all the slopes and terraces of this part of the island. There are other less extensive beds over Flying Fish Cove, and also at several points along the eastern edge of the plateau. Probably, when the islands were still low and not covered with forest, they formed the homes of myriads of sea birds, and the guano thus formed, after undergoing alteration, mainly through loss of its organic matter, gave rise to the hard phosphatic rock now existing. At the same time the limestones on which it rests have often been phosphatized, and lumps of coral consisting mainly of phosphate of lime are sometimes found. The extensive accumulations of guano which must have taken place point to a time when the rainfall was much less than at the present day, a condition which may, at least in part, have been dependent on the circumstances that the islands were low and probably free from forest.

On the eastern rim, between Phosphate Hill and Ross Hill, there are several smaller elevations, all presenting similar characters, viz.: having on their seaward side a steep talus slope or low cliff, a flat top, and a moderate declivity on the inland side. Between the hills both on the north and east coasts, the rim of the plateau varies a good deal in character. As a rule, its outer edge is marked by a kind of rampart of lines of limestone pinnacles separated by channels, but sometimes it descends by a gentle slope; in either case, beneath the cliff or the slope there is always a steep talus-strewn declivity passing down to the first terrace.

The northern part of the plateau within the elevated rim is particularly characterized by the presence of numerous low hills (about 50 feet), with more or less flat tops covered with blocks and pinnacles of limestone. Further south there are several step-like ridges, running in a generally east-and-west direction; their southern face is covered with blocks of limestone, composed mainly

of rather fresh-looking corals. In a few places similar limestones form extensive reefs, cut up into deep channels and holes. These reefs, when covered with thick bush, form almost impenetrable obstacles.

The Upper Slopes and Terraces.

As already mentioned, there is beneath the edge of the plateau a steep slope usually covered with talus, but where the rocks composing it are exposed they are found to consist of foraminiferal and coral limestones, and are often full of angular fragments of older limestones. Beneath this slope is a level terrace varying in width from a few yards to a quarter of a mile or more, and bounded on the seaward side by a second steep declivity, or in places by an actual cliff. The rocks comprising it usually show very distinct traces of coral, and sometimes seem to be entirely composed of it. This slope is absent on the southern side of the island.

The next terrace also varies considerably in width; on its outer margin there is usually a broad belt of pinnacles of limestone separated by channels. In the neighbourhood of Steep Point, it rises into a rounded hill covered with blocks of phosphate of lime. This hill must have formed a small islet at the time the foot of the second inland cliff was washed by the sea. In other places there is a channel 40 or 50 yards wide running parallel to the edge of the cliff; the inner side is formed by a cliff 30 or 40 feet high, the outer by walls and pinnacles separated by branching channels, the floor of which, like that of the main channel, is perfectly level. Towards the sea there is a steep slope covered with blocks of limestone. When the sea was 350 to 400 feet higher than at present, this channel formed a sort of canal in the reef parallel to the coast.

Beneath the terrace just described comes the first inland cliff, by far the most conspicuous feature of the island. Usually it has a vertical, or nearly vertical, face, and it is especially well-marked at the headlands. Its summit is from 250 to 300 feet above the sea. In several places about 150 feet above the shore platform there are distinct traces of wave action, the most notable being the presence of caves along this line. In some cases beneath this point, instead of a vertical face, we find a steep slope of limestone with coral in position of growth, apparently the remains of a narrow fringing reef, founded upon and partly composed of talus. That the elevation of this cliff has been of an intermittent character is further shown by the fact that where the slopes of the island are gentle and no high cliff has been formed, there is either a succession of minor cliffs separated by terraces and partly built up of coral rock, or merely a slope with ledges of coral limestone. Although these minor cliffs and ledges may be continuous for some distance in any given locality, they do not always correspond to those found a few miles off. It must also be noted that the geological structure and even the origin of this cliff are not everywhere the same, a point that will be referred to more fully below.

The shore terrace slopes gently down from the foot of the first

inland cliff to the sea-cliff, which is from 15 to 30 or more feet high, and is often undercut by the waves to a remarkable extent, so that it sometimes overhangs more than 20 feet. The inland side of this terrace is often covered with pinnacles of rock similar to that of the cliff above, and once formed part of the foreshore planed down by the waves. Near the sea the terrace is clearly a raised fringing reef resting on a foundation of talus; the corals are often very fresh in appearance. In some localities this platform has been cut into the older rocks (orbitoidal limestones, basalts, etc.) which form the basis of the island, and in such places small streams may occur, the water being held up by the volcanic rocks. The point where these are best developed is on the east coast, where there are two or three muddy brooks and a small fall of excellent water, which gushes out over a bed of basalt just above high-water mark.

On the south, where the cliffs are exposed to the full force of the swell produced by the south-east trade wind, which blows most of the year, the coast scenery is very fine. The cliffs are cut into numberless narrow inlets, and their summits are often completely bare of vegetation for some distance from the sea. Blowholes are very numerous, and several columns of spray rising high above the trees may often be seen at once.

At various points round the coast there are shingle beaches. The most important of these are that in Flying Fish Cove and West White Beach, at both of which landing is fairly easy. There are also two or three others on the north coast, several on the east, and one or two towards the northern part of the west coast; most of these are small and shut in by cliff, and are covered at high-water.

The above is a brief account of the usual plan of the island, but there are several localities in which considerable divergences from this occur. One of these is Steep Point, where a deep fissure, forming a narrow valley, has cut off an angle of the first inland cliff, and the portion thus isolated has tilted forward so that the usual shore platform, if it ever existed, has been carried beneath the sea, and the headland, which is 150 to 200 feet high, is, in fact, part of the first inland cliff.

Again, on the east coast, near North-East Point, extensive slips or faults have taken place, the result being that the ordinary terraces are replaced by a single precipice 500 to 600 feet high, the foot of which is covered by talus of enormous blocks of limestone. In this case the edge of the island as far back as the second inland cliff has slipped down beneath the sea, and has helped to build up the foundation upon which the reefs now forming the shore terrace were built. Nearer North-East Point the slip was less extensive, and the slipped mass here forms the first inland cliff, on the top of which there are several step-like ridges running parallel to its edge, and marking minor dislocations. The rock comprising both this cliff and the precipice further south seems to be almost wholly a mass of orbitoidal limestone, the flat joint faces of which give it a very

characteristic appearance; the cliff on the southern side of Egeria promontory is similar, and the small cliffs resulting from successive steps can be plainly seen.

At the western end of the island the upper cliffs are replaced by gentle slopes, and even the first inland cliff is ill defined, except towards North-West and Egeria Points. Another characteristic feature of this region is the occurrence of several valleys running down to the sea in a generally south-western direction. These first commence as a shallow depression at about 400 feet, but, as they are followed, deepen to a narrow gorge which cuts through the first inland and sometimes the sea cliff also. The scenery of these valleys is the most picturesque on the island, and reminds one a little of the dales in the Mountain Limestone in the Peak District. The floor of these valleys is generally formed of volcanic rock (basalt), and in the wet season is occupied by a small stream, which descends to the sea by a succession of falls and rapids; but at the time of my visit water was only found in the northernmost valley.

The last locality to be described is Flying Fish Cove, by far the most important, because it seems to supply the key to the structure of the island as a whole. In the large-scale map of this district given, it will be observed that at this point the sea-cliff is interrupted, and its place taken by a long curved stretch of white shingle beach, in front of which a broad fringing reef stretches from one end of the cove to the other. Behind the beach is a nearly level platform, composed mainly of blocks and fragments of coral mingled with talus from the cliff above. This level has been, for the most part, cleared and planted with cocoanut-palms, fruit-trees, and vegetables, and is the site of "Clunies-Ross Settlement," which consists of some nine or ten houses, workshops, and stores. The cliff joining the back of the cove is about 500 feet high in the middle, but decreases in height towards the ends, and towards the north the slope becomes less steep. For the greater part of its length it consists of alternations of low, more or less vertical cliffs, with steep talus slopes; but towards the southern end the upper part forms overhanging precipices of 200 feet or more in height, while the lower portion is covered by a talus slope of limestone blocks, often as large as a fair-sized cottage. Some of these lie far out on the reef. In this cliff and in its immediate neighbourhood we have almost the only section from which it is possible to get an idea of the nature of the foundation upon which the upper reefs have been established; almost everywhere else the central portion of the island is concealed by the investing covering of more recent limestones which have formed round the island, either as sediment derived from the higher coral masses or as reefs which have grown on the slopes of the island during its elevation. The circumstance that nearly all the rocks of the island are white limestones, often largely made up of fragments of older beds or containing fossils derived from them, renders the interpretation of the facts observed a matter of great difficulty. It will not be necessary, however, here to enter into details of the geology of the island, and only a brief

sketch of the structure of the neighbourhood of Flying Fish Cove is given.

Round the greater part of the cove, about half-way up the cliff, there is a thick bed of yellow foraminiferal limestone, the nearly vertical face of which is from 15 to 60 feet high. Beneath this, and apparently penetrating its lower surface, are several masses of volcanic rock, mainly basaltic. Above the limestone is another bed of basalt, upon which there are thick bands of palagonite tuffs, and occasionally traces of basalt above these again; but the upper slopes are so thickly covered with soil and fallen blocks of limestone, that it is difficult to determine the exact structure of this part of the cliff. About the middle of the cove the hard limestone is found at a rather higher level, apparently the result of faulting, but its relations to the volcanic series are the same. Southward of this the bed dips downward towards the shore, and the basalt and tuffs resting upon it disappear abruptly, their edges being overlapped by hard white limestone with *Orbitoides*. This rock forms the upper 50 feet or so of the cliff throughout its length, and on the summit occurs in low cliffs, ridges, and pinnacles. At the southern end of the cove it thickens out to a cliff some 250 to 300 feet high, the lower part of which is penetrated by masses of basalt. Above the cliff to the southward this limestone is found on the eastern and western sides of a broad belt of basalt, which forms a series of rounded hills with valleys opening towards the sea. On the eastern side its base is about 500 feet above the sea; on the west, where it is largely concealed by the more recent deposits forming the cliffs and terraces representing the first inland cliff, it is only 300 to 350 feet. No doubt this limestone completely covered the basalt, but has been removed by denudation; and, in fact, further south the volcanic rock is completely concealed by limestones.

Further inland, above the orbitoidal limestones comes the steep slope of the second inland cliff, which is here largely composed of corals; shells of mollusca are also found, and there are some beds of foraminiferal limestone without *Orbitoides*. Above this cliff is a long slope with lines of limestone pinnacles parallel to its edge, and above this, again, the upper inland cliff, or rather slope. Along the foot of this there are some ridges of coral limestone; but towards the summit it rises into rounded hills of dolomitic limestone, with a great many blocks of phosphate of lime here and there. These piles are probably the remains of islets along the edge of the lagoon (now the plateau) before the first elevation of the island took place.

The history of the island, as far as it can be made out, seems to have been as follows:—At first, at no great depth, there was a submarine bank upon which numerous foraminifera, including *Orbitoides*, lived, and the shells of which formed thick beds of limestone. The foundation of this bank was volcanic, and from time to time lava was erupted through and upon the limestones: the occurrence of thick bands of palagonite tuff indicate that the eruptions were submarine. Some elevation took place, and the

beds of tuff became consolidated by the infiltration of lime. In the next place, the whole was covered with thick beds of white limestone crowded with large *Orbitoides*. These strata seem to have overlapped the edges of the beds of tuff, and in places it can be seen that they dip away from the central mass. The deposits resting on the orbitoidal limestones are for the most part covered with recent accumulations, but they appear to have been mainly foraminiferal limestones.

Upon the foundation thus prepared extensive reefs grew up and formed an atoll-shaped group of islands, the reef flat and islands being now represented by the raised rim of the plateau and the hills rising from it, the lagoon by the central plateau itself. The rounded hills and lofty pinnacles found within the raised margin are probably the remains of knolls and masses of coral growing up in the lagoon, such as may be seen in the Cocos-Keeling Islands at the present day. The height of the hills over North-West and South Points may be accounted for by supposing either that they are points of local elevation greater than that affecting the main mass of the island, or that they represent the higher parts of the bank, upon which reefs were formed before the greater part of it was near enough to the surface for the growth of reef corals. During the formation of these higher reefs, the material derived from their wear, mingled with the remains of organisms living around the coast, formed thick deposits of limestone upon the flanks of the island.

The first important movement which took place seems to have resulted in the elevation of the northern and eastern sides of the island, the south and west probably remaining submerged. At this time the reefs forming the second inland cliff grew round the north and east coast, and probably some of the ridges of coral limestone running across the middle of the island were formed near the new shore-line on the side of the lagoon.

The next extensive elevation affected the whole area equally, and along the new shore-line the second inland cliff was cut back into the reefs just formed, or even in some places into the central foraminiferal limestones. Subsequently a series of movements of elevation led to the formation of the first inland cliff, or, on the more gentle slopes, to the succession of small cliffs and ridges of coral rock which represent it.

During these various movements much slipping and faulting took place round the island; the effects of this at Steep Point and North-East Point have already been described. As a result of this, and of the action of the waves around the coast, a submarine talus slope was formed, upon which a fringing reef was established, and at the next elevation this was converted into the shore terrace, while its margins were cut back into the present sea-cliff. Finally, as already mentioned, a reef is now growing around the coast which some day may form yet another raised terrace round the island. It is a point of some interest that Mr. Andrew Ross, during the eight or nine years he has been residing on the island, has noted the

occurrence of two slight earthquakes, the more severe occurring in October, 1895; this was followed by heavy falls of rock from the cliffs.

II.—Structure of a Coral Reef.—Report of the Committee, consisting of Professor T. G. Bonney (Chairman), Professor W. J. Sollas (Secretary), Sir Archibald Geikie, Professors J. W. Judd, C. Lapworth, A. C. Haddon, Boyd Dawkins, G. H. Darwin, S. J. Hickson, and Anderson Stuart, Admiral Sir W. J. L. Wharton, Dr. H. Hicks, Sir J. Murray, Drs. W. T. Blanford, C. Le Neve Foster, and H. B. Guppy, Messrs. F. Darwin, H. O. Forbes, G. C. Bourne, and J. W. Gregory, Sir A. R. Binnie, and Mr. J. C. Hawkshaw, appointed to consider a project for investigating a Coral Reef by boring and sounding.

THE boring into the coral reef at Funa Futi, under the superintendence of Professor Edgeworth David, was carried down to a depth of 643 feet. After he had quitted the island to return to Sydney the work was continued until, owing to a breakdown of the apparatus, it finally ceased at a depth just short of 700 feet. The cores obtained during the work have been forwarded to England, and are now being worked out under the supervision of Professor Judd in the laboratory of the Royal College of Science at South Kensington. A brief summary of the results down to 643 feet was presented to the Royal Society on November 25, 1897, and will be found in their "Proceedings." According to the survey of Funa Futi and the neighbouring seas made by Captain Field, of H.M.S. "Penguin," it appears that the shape of the former is that of a cone with a rudely elliptical base rising with a gradual slope from the ocean floor at a depth of about 2,000 fathoms, and forming a kind of mural escarpment for the last 750 feet (approximate). When the whole party had returned to Sydney, Professors David and Stuart, after discussing the question of renewing the attempt to pierce the reef, the bottom of which, from the change of slope mentioned above, they thought must lie within 800 feet of the surface, prevailed on the authorities of the Department of Mines, Sydney, to lend plant and workmen in order to continue the old borehole, and, if possible, to put down another one in a shallow part of the lagoon. Application was made to the Admiralty by the Royal Society, and permission was given for the members of the expedition and the plant to be conveyed from Suva to Funa Futi and back by H.M.S. "Porpoise." The expedition has been at work during the summer, and intelligence of the result will doubtless have reached England during the autumn. Until this arrives, and the study of the materials already in this country has been completed, it would be premature to express any opinion of the theoretical bearing of the results obtained by the very successful operation undertaken in 1897.

III. — The Fossil Phyllopoda of the Palæozoic Rocks. Fourteenth Report of the Committee, consisting of Prof. T. Wiltshire (Chairman), Dr. H. Woodward, and Prof. T. Rupert Jones (Secretary). (Drawn up by Prof. T. Rupert Jones.) [1]

§ I. In our First Report on the Palæozoic Phyllopoda (presented to the Association in 1883) the genus *Dithyrocaris* was included in the Tabular List at p. 216 of the Reports for 1883 (1884), as being "ridged along the back (like *Apus*)," and as "being ridged and sometimes prickled." It was referred to as occurring in Carboniferous and Devonian strata.

In the Fifth Report (made in 1887), at pp. 63–66, we enumerated all the known and reputed species of the genus. Thanks to the obliging courtesy of friends and correspondents, we are now enabled to state that we can distinguish the following species found in the British Islands and elsewhere:—

		Scotland.	Ireland.	England.	Belgium.	Germany.	Canada.	U.S.A.
Carboniferous.								
Carapaces.	1. *Dithyrocaris glabra*, H. Woodward & R. Etheridge, jun.	*						
	2. ,, *ovalis*, W. & E. ...	*						
	3. ,, *granulata*, W. & E.	*						
	4. ,, *testudinea*, Scouler ...	*		*				
	5. ,, *Scouleri*, M'Coy ...	*	*					
	6. ,, *funiculata*, sp. nov. ...	*	*					
	7. ,, *Colei*, Portlock ...	*	*					
	8. ,, *tricornis*, Scouler ...	*		*				
	9. ,, *orbicularis*, Portlock		*					
	10. ,, *tenuistriata*, M'Coy...		*	*	*			
	11. ,, *Youngii*, sp. nov. ...	*						
	12. ,, *striata*, Woodward ...	*						
Tailpieces.	13. ,, *lateralis*, M'Coy ...	*		*				
	14. ,, *Neilsoni*, sp. nov. ...	*						
	15. ,, *Dunnii*, sp. nov. ...	*		*				
	16. ,, *carbonaria*, Meek & Worthen							*
	17. *Rhachura venosa*, Scudder							*
Devonian.								
Carapace.	18. *Dithyrocaris Belli*, Woodward...						*	
Tailpieces.	19. ,, *Kochi*, Ludwig... ...					*		
	20. ,, *breviaculeata*, Ludwig					*		
	21. ,, (?) *Jaschei*, Römer ...					*		
	22. ,, *Kayseri*, Clarke ...					*		
	23. *Mesothyra Occani*, Hall & Clarke...							*
	24. ,, *Neptuni*, Hall							*
Carboniferous.								
	Gastric Teeth	*	*	*				

[1] Being a paper read before the British Association for the Advancement of Science, Section C (Geology), Bristol, September, 1898.

Dithyrocaris has a clypeiform test; at all events most of the specimens have a shield-like test, readily dividing into two moieties or valves; but some specimens seem to support the idea of having been able to fold the two sides together. The moieties are often separate, and some are too convex to have formed a quite flat shield; some have the lateral edges turned sharply downwards and inwards.

The valves, or two lateral moieties, were united along their dorsal edges simply; several specimens, however, had a dorsal rugose ridge-plate, over-riding, narrow, and longitudinal (somewhat like that described as an intervening plate in *Mesothyra* by Hall and Clark in 1888), ending in a posterior spine.

The valves are sub-oblong, straight along the middle two-thirds of the dorsal border, and elliptically curved ventrally; more or less rounded at the ends, with a median hollow or notch at their junction on the front and hind borders. The antero-dorsal region ends with a blunt angle or a short process; and the postero-ventral with a strong, sharp, trigonal spine.

The straight hinge-line is defined by two small dorsal notches. The ventral border has a striated, serrated, or fringed margin, either on its posterior moiety or throughout its extent.

The surface of each valve bears one longitudinal (*meso-lateral*) ridge, and sometimes others parallel; also short ridges (*cephalic*) over the gastric apparatus, and slighter ridges (*nuchal*) near the top of the dorsal ridge, all more or less rugose.

Scattered granulations and tubercles are often present on some parts of the valves, also lines and reticulations.

Granulation is feeble and sparse on *D. glabra* and *D. ovalis*; strong and abundant on *D. granulata*. Small prickles, rising from the meshes of a reticulation, are scattered over *D. tricornis*, *D. Colei*, and *D. orbicularis* (?). A system of oblique transverse lines characterizes *D. testudinea*. A feeble reticulation is traceable on *D. funiculata* and *D. Scouleri* (?). Longitudinal striæ mark the surface in *D. Belli* and *D. striata*; and *D. tenuistriata* and *D. Youngii* have longitudinal costulæ.

In consideration of certain differences in the carapaces, we separate Nos. 10 and 11 of the Table, at p. 28, from *Dithyrocaris*, as *Chænocaris tenuistriata* and *Ch. Youngii*, the carapace being bivalved and *gaping*. There is also an obscure Devonian form, from Saalfeld, to which we refer as *Chænocaris Richteriana*. We regard No. 12 as having a *closed* bivalved test, and therefore designate it as *Calyptocaris striata*.

We have had the opportunity of studying an old Apus-like fossil[1] labelled 'Burdiehouse.' It shows a small circular carapace (measuring 15 by 13 mm.), with strong postero-ventral angles, and distinct meso-lateral ridges leading to them; also a slightly curved depression in the middle of the front border, and a granulated margin throughout.

Mr. E. J. Garwood, F.G.S., who is a member of the British Association Committee for defining the zones in the Carboniferous

[1] *Hibbertia orbicularis.*

rocks, has just now forwarded for examination a very interesting collection of the remains of *Dithyrocaris*[1] from excavations in the shales of the Millstone-grit series at Eccup, Yorkshire.

§ II. In the "Proc. United States National Museum," vol. xix (1897), Mr. C. Schuchart has a paper "On the Fossil Phyllopod Genera *Dipeltis* [Packard *emend.*] and *Protocaris*." The latter was noticed in our Report to the Association for 1889, p. 64, and in our Ninth Report (for 1891), p. 300. The original genus *Dipeltis* was established by A. S. Packard, in the "Memoirs of the National Academy of Sciences," vol. iii (1885), Mem. xvi, p. 145, pl. v, figs. 2, 2*a*, as one of the Carboniferous Xiphosura of North America. In the December number of "Natural Science," 1897 (vol. xi, p. 401, figs. 2–5), in his paper on "Fossil Apodidæ," Mr. H. M. Bernard follows Mr. Schuchart in regarding the *Dipeltis*, as defined by the latter, as a Phyllopodous Apus-larva. Mr. C. J. Gahan, however, in "Natural Science," January, 1898, pp. 42–44, points out that it is really a larval form of the Blattarian insect *Etoblattina*, described and figured by H. Woodward in the GEOLOGICAL MAGAZINE, 1887, p. 433, Pl. XII.

§ III. With reference to the Bohemian *Estheriæ* mentioned at p. 4 of our Report for 1893 (Tenth, 1894), Dr. Anton Fritsch has informed us that they were named by him in the "Sitzungsb. k. böhm. Gesellsch. Wissen.," 1894: No. 1 being *Estheria triangularis*, Fr.; No. 2, *E. cyenea*, Fr.; No. 3 [and No. 4 ?], *E. palæoniscorum*, Fr.; and No. 5, *E. calcarea*.

IV.—THE COMPARATIVE ACTIONS OF SUBAËRIAL AND SUBMARINE AGENTS IN ROCK DECOMPOSITION. By THOMAS H. HOLLAND, A.R.C.S., F.G.S., Geological Survey of India.[2]

IN Europe nearly all crystalline and igneous rocks of any considerable age show signs of hydrous decomposition, which by the microscope can generally be traced far beyond the limits of the very evident superficial crust of weathered products: in some cases, like the peridotites, the changes due to hydration, even in rocks of Tertiary age, have resulted in a practically complete alteration of the original constituents. In working over various parts of Peninsular India, the writer has been struck by an almost constant absence of any but the most superficial traces of hydration, even in minerals like olivine and nepheline, which are so noticeably susceptible to the action of water. As in all tropical and moist climates, however, a complete and rapid superficial decomposition is shown by most of the rocks, and in some areas they are found to be changed into a ferruginous clay, which, though forty or fifty feet thick, is found to retain the characteristic macroscopic structures of the original rocks. In some districts, where the atmosphere is always warm, and during the monsoon season highly charged with moisture without great precipitation of rain, the rocks are

[1] For which we propose a new species, *D. insignis*.

[2] Read before Section C (Geology), British Association, Bristol, 1898.

similarly decomposed at the surface, but, on account of the limited amount of running water, the lime is retained in the decomposition products, and forms a concretionary 'kankar.'

In all these cases, however, although the action of the atmosphere is so striking, the results are purely superficial, and a specimen of rock taken from within a few inches of the clay products seldom shows a trace of hydrous decomposition, even in thin sections under the microscope. This is just as true for such delicate minerals as olivine and nepheline as for the commoner silicates. In many of the basic dykes, certainly pre-Cretaceous and probably Lower Palæozoic in age, the absence of serpentine is so complete that unusual precautions are often necessary for the determination of the olivine, whilst in the numerous occurrences of dunite throughout the Madras Presidency serpentine is extremely scarce. In a nepheline syenite recently discovered in the Coimbatore District, and at least of Cuddapah age, the nepheline on microscopic examination shows mere traces of alteration along the fracture cracks.

In the light of European experience, where most of our petrographical data have been established, the peculiarities of the Madras rocks call for some special consideration, and the object of this paper is to suggest that the probable explanation of the peculiarities now referred to arises from a contrast of the geological histories of the two areas. In Europe all, or nearly all, the rocks have been submerged below the sea during the later geological periods; in South India there is no evidence beyond the immediate precincts of the coastline of any depression below sea-level since Cuddapah (probably Lower Palæozoic) times. In Europe, therefore, the features generally attributed to weathering are the compound effect of submarine and subaërial action; in South India the former class of agencies has not affected the rocks now exposed, and the remarkable freedom from hydration which they show suggest that the action of the atmospheric agencies is purely superficial.

Taking into consideration the presence of lime carbonate and other salts, with a larger proportion of carbonic acid and the great pressure under which sea-water attacks a submerged rock-mass, it is theoretically to be expected that submarine agencies are more potent means of decomposition than those of the atmosphere; but these South Indian observations tend to show that serpentine and other forms of hydrated products within rock-masses are due only in a very limited degree to true weathering.

The products of atmospheric action are removed from the rock surface as fast as they are formed, and deeper portions are *pari passu* brought to the surface. It is not improbable that it is on account of this denudation, which has proceeded without known interruption for so many geological ages, that relatively deep-seated portions of the earth's crust have been brought to the surface in Madras, and that the crystalline rocks there met with at times present peculiarities for which European experience hardly prepares us.

REVIEWS.

I.—Wachsmuth and Springer's Monograph on Crinoids.[1]

Fifth Notice.

"THE Anal plates," say Messrs. Wachsmuth and Springer, on p. 124, "bear a most important part in the phylogeny of Palæozoic Crinoids, and are also of high importance in respect to classification." This admission necessitates careful inquiry into the homologies of those plates. The views of our authors "have undergone considerable modification"; but the discussion need not be complicated by allusion to earlier publications, however great their quondam value, except when they have been cited as evidence in the present Monograph.

The term 'anal plates' (*analia*) is now restricted by Wachsmuth and Springer to such plates of the posterior interradius as (1) "are directly or indirectly connected with the anus," (2) "take part in the dorsal cup; the others being plates of the anal tube" (tube-plates). The first restriction forbids the application of the term to such of the posterior interradials as are serially homologous with those in other interradii of the individual; it is practicable and natural. The second restriction merely evades one difficulty and creates another. For instance, in such a Camerate genus as *Glyptocrinus* (Fig. 1), the plates of the cup proper merge so gradually into those of the tube proper that no two observers would agree upon the line to be drawn between them in any one specimen; further, owing to the gradual growth of interbrachials, plates that are free tube-plates in the young become fixed cup-plates in the adult, just as free brachials become fixed. Again, in such Inadunate genera as *Delocrinus*[2] (Fig. 9) and *Ulocrinus* (Fig. 3), it is hard to see, from

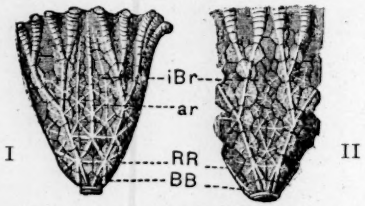


Fig. 1.—*Glyptocrinus decadactylus*. I. From posterior interradius (after W. and Sp.), showing *ar*, the anal ridge, dividing the interbrachials, iBr. II. Another interradius, for comparison (after Meek).

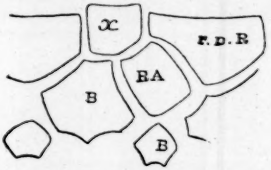


Fig. 2.—Anal area of '*Ulocrinus*' *Blairi* (based on Miller & Gurley's figure). This is not *Ulocrinus*, but represents a stage between *Eupachycrinus* and *Ulocrinus*. It strongly supports the homology here adopted for the anals of *Ulocrinus*.

[1] The North American Crinoidea Camerata. By C. Wachsmuth and F. Springer. Mem. Mus. Comp. Zool. Harvard, vols. xx and xxi, containing 838 pp. and 83 plates. (Cambridge, U.S.A., May, 1897.) For First, Second, Third, and Fourth Notices, see Geol. Mag. for June, July, September, and November, 1898.

[2] Miller and Gurley, 1890 = *Ceriocrinus* White pars, 1880, non Desor, 1845, nec König in tabb. ineditis.

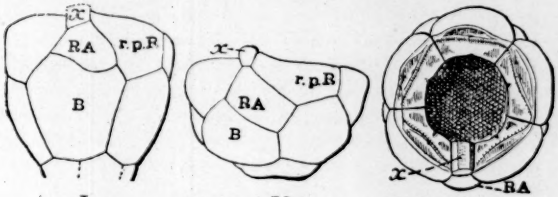


FIG. 3.—*Ulocrinus* (diagrammatized from Miller & Gurley; × $\frac{2}{3}$).
I. Posterior view of cup of *U. Buttsi*. Note that *x* is not so far removed from post. B. as in II, *U. kansasensis*, posterior view. III. The same seen from above, showing the upper surface of *x*, which must have supported tube-plates.

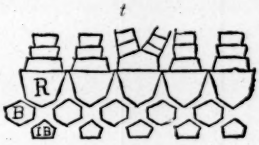


FIG. 4.—*Merocrinus*.

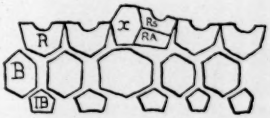


FIG. 7.—*Homocrinus*.

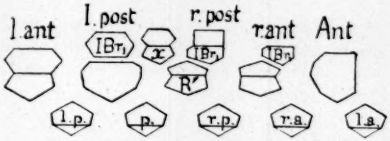


FIG. 5.—*Heterocrinus bellevillensis*. (Outline after W. & Sp.)

FIG. 8.—*Zeacrinus*.

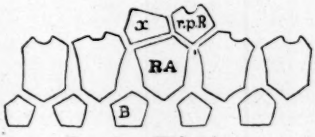


FIG. 6.—*Hybocrinus*. (Outline after W. & Sp.)

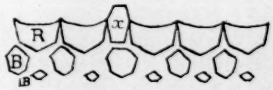


FIG. 9.—*Delocrinus* (= *Ceriocrinus*, White).

In all these analyses (Figs. 4–9) the orientation is that of Fig. 5. The plate *x* of Figs. 3, 5, and 6 is called *t* by W. & Sp. In Fig. 4, *t* denotes the anal tube.

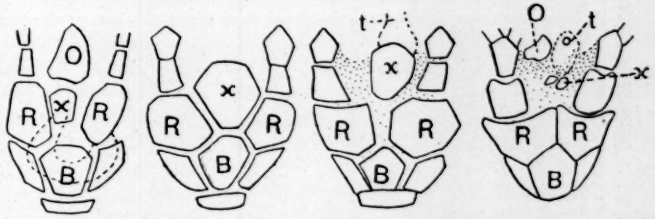


FIG. 10.—Migration of the anal in *Antedon* (after W. B. Carpenter & M. Sars). Originating between the radials, the plate *x* gradually moves upwards, eventually atrophying till only two small fragments remain at the base of the anal tube (*t*). The dotted lines in the left-hand figure show the course of the gut.

Wachsmuth and Springer's own diagrams 16 and 17 on p. 131, and still more from specimens of various species, why a plate, here marked *x*, occupying almost exactly the same position with regard to the radials, should be called 'anal' in the former and 'tube-plate' in the latter; or why the plates marked *t* in their diagrams of *Hybocrinus* (8; *x* in our Fig. 6) and *Poteriocrinus* (15; *rt* in our Fig. 14) should not be held to form part of the cup, since they abut by their sides on plates that are admittedly cup-plates. Moreover, according to this definition, the posterior interradial of the larval *Antedon* is an 'anal' so long as it lies between the radials, but a 'tube-plate' so soon as it passes above them (Fig. 10). Position, then, affords no practicable or consistent guide. But the proximal tube-plates rarely possess any other feature not shared by the fixed anals. Therefore the distinction between 'anals' and 'tube-plates' rests on no morphological foundation, and the terms, though convenient for general descriptive purposes, are out of place in an argument as to homologies. And yet, as will be seen, it is for the purpose of such an argument that the imaginary distinction has been emphasized.

Anals appear in the cup in response to the need for widening the posterior interradius so as to allow room for the rectum internally, and so as to support the anal tube in which the rectum is prolonged externally. We can conceive four ways in which this widening may be effected: (1) by simple widening of plates already in existence, as in Platycrinidæ; (2) by splitting or duplication of plates, as the proximal interbrachial of Actinocrinidæ, which is represented by two plates in the posterior interradius; (3) by intercalation of a fresh plate or line of plates, unrepresented in the other interradii, as, possibly, the large anal by which Hexacrinidæ are distinguished from Platycrinidæ, and the vertical row of anals in Batocrinidæ and Dimerocrinidæ (=Thysanocrinidæ, W. & Sp.); (4) by the lateral displacement or partial atrophy of adjacent radials, permitting other elements to support the tube, as in *Hybocrinus*, where the right posterior superradial is pushed aside and the proximal plate of the tube rests on the enlarged r. post. inferradial, which thus becomes a 'radianal' (Fig. 6).

Now there is no difficulty in any of these conceptions. The examples just quoted are taken from the present Monograph, and probably would be accepted by anyone competent to judge. The difficulty lies in the application to certain cases, notably to the anal area of the Inadunata. Nearly the whole of the sixteen pages on the anal plates is devoted to a discussion of their evolution in that group.

Most of the Inadunata, especially those with a large anal tube or sac, as well as many of the Flexibilia Impinnata (=Ichthyocrinoidea), possess in the anal area of the cup from one to four plates. These are not introduced at haphazard, but bear to one another and to the other cup-plates a definite relation. What the precise relation is, though agreed upon in some cases, remains an open question in others. The fullest development is seen in *Poteriocrinus* and the immediately allied genera (Fig. 14). Here the anals in

contact with cup-plates are four in number: a plate (*x*) resting on the truncate upper margin of the posterior basal; a plate (RA) resting between the posterior and right posterior basals, abutting against *x* on the left and the right posterior radial on the right, and

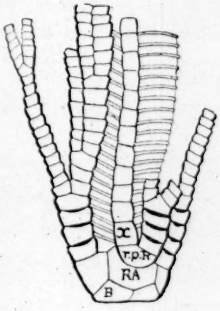


FIG. 11.—*Iocrinus subcrassus.* (Outline after W. & Sp.)

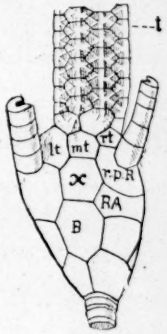


FIG. 12.—*Dendrocrinus longidactylus.* (After J. Hall.)

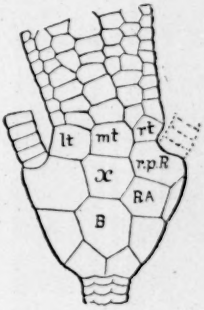


FIG. 13.—*Dendrocrinus Casei.* (Diagrammatized from W. & Sp.)

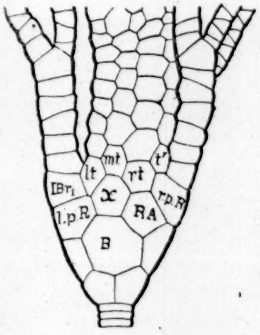


FIG. 14.—*Poteriocrinus* (*Scaphiocrinus*) *elegans.* (After W. & Sp.)

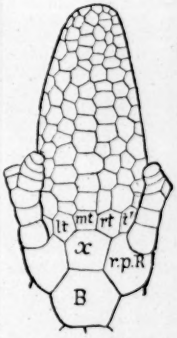


FIG. 15.—*Cyathocrinus longimanus*, anal area. (Diagrammatized from Angelin by W. & Sp.)

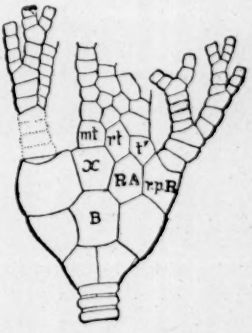


FIG. 16.—*Parisocrinus.* (From MS. drawing lent me by Wachsmuth.)

supporting by its upper margin a tube-plate; this latter (*rt*) rests between *x* and r. post. R, and is followed by a vertical series of similar plates; on the left of *x*, and resting on l. post. R, is a rather smaller tube-plate (*lt*), also supporting a vertical series; *x* itself supports a median vertical series, of which the proximal plate may be distinguished as *mt*.

As to the homologies of these plates, all are now agreed that RA represents the right posterior inferradial, and my name for it, 'radianal,' has won general adoption. This plate, as Wachsmuth and Springer rightly insist, is always found to alternate with the underlying basals; in the early stages of phylogeny it supported a superradial of equal width, but that became squeezed to the right as the anal plates developed: compare *Heterocrinus* (Fig. 5) and

Dendrocrinus (Figs. 12 and 13). Thus RA altered in outline, and was brought into contact with other anal plates; first with *x* (as in *Dendrocrinus* and *Homocrinus*, Fig. 7), then also with *rt* (as in *Poteriocrinus* and allies, Fig. 14), while it might even support a third plate to the right of *rt*, as in some species of *Parisocrinus* (Fig. 16). In some later genera, such as *Zeacrinus* (Fig. 8), RA increased in size and *rt* was again raised above the level of the cup; while in *Ulocrinus* (Fig. 3) the plate *x* also was (to use Wachsmuth and Springer's expression) "crowded out by the large radianal." In other genera, however, RA became reduced and finally disappeared (e.g. *Delocrinus*, Fig. 9).

To turn to *rt*, the plate which in *Poteriocrinus* (Fig. 14), *Atelestocrinus*, and similar forms rests between *x* and r.post.R and on RA, and therefore in the limits of the cup: it is admitted that this is subsequently pushed up out of the cup (e.g. *Zeacrinus* and *Ulocrinus*, Figs. 8 and 3); further, that it was at an earlier stage resting on the left shoulder of r. post. Rs (e.g. *Dendrocrinus* and *Homocrinus*, Figs. 12, 13, and 7). To this extent I am in full accord with Messrs. Wachsmuth and Springer, and would point out that this admission stultifies their distinction between anals and tube-plates, and allows that a tube-plate can sink down into the cup.

In *Heterocrinus* (Figs. 5 and 17) and *Ectenocrinus* a single plate at the base of the tube rests on the shoulders of the two posterior radials, which are here not separated by any anal plate. This proximal single plate is considered by Wachsmuth and Springer to "take practically the place of the three plates [*lt*, *rt*, and *mt*] in *Dendrocrinus*." They also homologize it with the proximal tube-plate of *Hybocrinus*, which they say on pp. 129, 130, 133 has a similar position. This is a peculiar slip, for their own figure shows that this plate in *Hybocrinus* lies between the posterior radials on a large radianal (Fig. 6). They also homologize with this the proximal tube-plate of *Iocrinus* (p. 130) and apparently *Merocrinus* (p. 128); but this plate rests only on the right posterior superradial, absolutely outside the cup, and is removed by the whole height of that plate from the left posterior radial (Figs. 4 and 11). If, therefore, these homologies of Wachsmuth and Springer be accepted, it follows that the plate in question (*t* in their figures, *x* in ours) moved either from a position well above the radial circlet (*Iocrinus*) to one-half within it (*Ectenocrinus*), and finally entirely within it (*Hybocrinus*), or in the reverse direction. Either view is in direct contradiction to their statement on p. 132 that "there was no sinking of the plate *t*, which never moved from its place *above* the radials." That is another of those dogmatic general assertions which are useful to dispose of other people's theories, but are overlooked when the opinions of Messrs. Wachsmuth and Springer would be invalidated by them.

Rightly or wrongly (in the main, I believe, rightly) our authors have thus far explained the modifications of the anal area in Inadunata on principle 4 of the above list, namely, by the shifting, atrophy, and growth of certain constant elements. But

when they turn to the plate which they term *x*, the case is altered. They start off by saying that this "is the homologue of the 'special' or 'first' anal plate of the Camerata, and rests upon the truncated posterior basal." This begs the whole position : it is equivalent to an assertion that *x* is suddenly developed in that position, and that it cannot possibly be homologized with any plate that does not truncate the posterior basal. As they express it on p. 132, "As the tube became larger, the radials spread out [i.e. apart], and the vacant space thus formed was filled by a new plate *x*." This explanation, though involving a different, almost an opposing, principle, may possibly be correct. But one would like to have some attempt at proof. One would expect to be shown primitive genera in which *x* first appeared as a narrow strip separating the radials and gradually widening. But no evidence of the kind is adduced. As for the argument based on a supposed homology with certain Camerata (p. 130), it must be recognized clearly that the appearance of *x* in Inadunata stands in no genetic relation to the appearance in Camerata. If the plate *x* be not a primitive structure, then its acquisition by Inadunata must have been independent of anything that may have taken place in Camerata. The sole value of the comparison lies in its suggestion that what was possible in one group was also possible in the other. But among Inadunate genera themselves, *Carabocrinus* has an obvious additional supplementary plate in the anal area of the cup ; so that the possibility of a similar origin for *x* may be freely admitted. The probability, considering the persistence and frequency of the structure, is a different matter.[1]

Before accepting the above hypothesis as to the origin of *x*, we have to consider whether any other hypothesis has been proposed. Wachsmuth and Springer do in fact honour with courteous and

[1] Wachsmuth and Springer are wrong in thinking that I ever denied the general homology of *x* with any plate in the Camerata. In a passage, which they quote almost accurately, I once said, "it may be pointed out that, as interradials do not enter into the composition of the dorsal cup in any Fistulate, none of these [anal] plates can well be the homologues of interradials : in many of the Camerata actual interradials are present in the anal area, but in the Fistulata at least we must look elsewhere for the origin of the so-called 'anal' plates" (*Ann. Mag. Nat. Hist.* [6], v, p. 319). Having regard to the meaning at that time attached to the term 'interradials,' it is clear that this merely meant that the anals of Fistulata could not have been developed by the modification of interradials, since no such elements pre-existed in that suborder, whatever might have been the case in Camerata. As to the course of evolution in Ichthyocrinidæ and the Camerata I said plainly (op. cit., p. 331) "I can express no opinion." It should have been impossible to infer from this that "Bather makes no reference to the anal plates of the Ichthyocrinidæ, but regards the anals of the Camerata as morphologically distinct from those of the Fistulata . . . . because they [Fistulata] possess no interbrachials." Again, I said that one had no right to regard the anal plate of *Antedon* as a mere interradial when there were no such plates in the other interradii. Wachsmuth and Springer seem to think this nonsense (p. 139) ; yet in the very next paragraph they say that the "large interradial plate" on which rests the anal tube of *Thaumatocrinus* "is not a special anal, for a similar plate is interposed between the radials of the other four sides." But this is the precise and legitimate converse of the very argument that they ridicule.

detailed discussion the hypothesis proposed by me in April, 1890,[1] and already criticized by them in February, 1891.[2] Long ago[3] I showed that their representation of my views was based on misconceptions, and again,[4] while accepting a part of their criticism, took the opportunity of re-stating my main position. By ignoring these protests in this Monograph, Messrs. Wachsmuth and Springer have readily gained credit for the apparent demolition of my supposed theories. A short re-statement of views already published by me may therefore be pardoned.

First, then, I have maintained "that the lowest, median, posterior plate of the ventral tube is always this same plate (*x*), whether it be right above the radials, as in *Iocrinus* and *Merocrinus*, resting on the radials, as in *Heterocrinus* and *Castocrinus*, between the radials but not in line with them, as in *Homocrinus* and *Dendrocrinus*, in line with the radials, as in *Botryocrinus* and *Cyathocrinus*, or rising above the radials again, as in the later Decadocrinidæ [*Delocrinus, Ulocrinus, Erisocrinus*] and in the larval *Antedon*."[5] Any objections to this homology based on an alleged difference between anals and tube-plates, or on the supposed impossibility of plates sinking into the cup, have already been proved groundless by the admissions and arguments of the objectors themselves. Moreover, since, as regards the other anal plates (RA, *rt*), it is now admitted that change took place by gradual modification of constant elements, the burden of proof should lie with those who imagine the sudden introduction of a new element. The statement of Messrs. Wachsmuth and Springer that "this question was not discussed by Bather," however true it may be for my paper of 1890, is hardly one that should have been published five years after I had discussed the question in more than one place. As for the 1890 paper, it must be remembered that the distinction between tube-plates and anals was not attempted by Wachsmuth and Springer till 1891.[6]

Secondly, I showed that if the known genera were arranged in a series according to the admitted evolution of the radianal, then one could trace a gradual descent of the proximal median plate of the anal tube, from the left shoulder of the right posterior superradial, to a position between the posterior radials and resting on the posterior basal; and, following this, the descent of *rt* from a yet

[1] Ann. Mag. Nat. Hist. (6), vol. v, pp. 319–334.
[2] Proc. Acad. Nat. Sci. Philadelphia, vol. for 1890, pp. 345–392.
[3] Ann. Mag. Nat. Hist. (6), vol. vii, pp. 480–489, June, 1891.
[4] Ann. Mag. Nat. Hist. (6), vol. ix, pp. 64–66, Jan. 1892.
[5] Loc. cit. ult.
[6] In reply to their present assertion (p. 128) that they had always regarded the proximal posterior plate of *Iocrinus* and *Merocrinus* "as a plate of the tube," and had "never made any statement from which he might infer that we thought it represented the plate *x*; yet he quotes us in his diagrams as if we had done so in 1879," it is enough to say that in plate xvi of their "Revision of Palæocrinoidea," 1879, the letters *an* are applied equally to the median line of tube-plates in *Iocrinus*, to the plate which they now and always have homologized with the proximal of those median plates in *Anomalocrinus* and *Hybocrinus*, and to the plate *x* of *Cyathocrinus*. With this before me, why on earth should I have introduced into a diagram doubts that had never occurred to me or anyone else?

higher position into the space between *x* and r. post. Rs, and resting on RA; and, eventually, the retracing of these steps, accompanied by gradual atrophy of all the anal plates.

Now the present Monograph admits, as we have seen, the sinking and re-raising of *rt*; it also admits the sinking of the proximal median plate of the tube from its high position in *Merocrinus* and *Iocrinus* to its low position in *Heterocrinus* and *Ectenocrinus*, and its still lower position in *Hybocrinus*; finally, it admits the sinking of plate *x* from a level more than half its height above the radials, as in *Homocrinus*, to the level of the radials, as in *Cyathocrinus*, and its final expulsion from the cup. What it does not admit is the possibility of a proximal tube-plate sinking down between the radials so as to touch the basal.

Thus the missing link in our chain of evidence is so slight, and is of a nature so in accordance with links that are known or admitted, that we must demand some good reason for the refusal to imagine this link and for the substitution of an entirely novel postulate. The only tangible argument to be found in the Monograph is given on p. 133, as follows:—"If it were true that the plate of *Iocrinus* to the left of the supraradial [r. post. Rs] passed down in later forms to the basals, it would mean nothing less than a partial revolution of the entire tube. This, however, is disproved by the structure as well as the palæontological development of the tube, which latter is generally composed of longitudinal rows of hexangular pieces, alternating in adjoining rows. In the earlier and simpler forms the tube consists of only five series, one to each interradius, that of the anal side resting upon plate *t*. Later on, as the tube grew larger, *a new row of plates was introduced with plate* x *supporting it.* When there are three series, as in *Dendrocrinus*, the third generally rests upon one side of the left posterior radial. The arrangement of the plates within the rows is so regular that if a sinking of the plate *t* had taken place it would certainly be indicated by some disturbance among the lower plates of the tube. In species where the tube has more than three rows, one or more of the primary rows dichotomize at some distance from the cup. [Compare Figs. 12 and 13.] The fact that the increase of the tube phylogenetically took place by the introduction of new rows of plates, is a strong argument in favour of our idea that the plate *x* is also a supplementary piece, and was introduced in the same manner as the plates which it supports."

If the statement which I have italicized were "a fact," it would be not merely "a strong argument," but absolute proof. But it is merely another phrasing of the very idea that is to be proved; and it is a phrasing that emphasizes the difficulty, for it maintains that the stout median line of the tube in *Iocrinus* and *Merocrinus* becomes replaced by another stout median line in *Homocrinus* and *Dendrocrinus*, and itself assumes a subsidiary position. No proof has yet been given that the added series may not be those starting from *rt* and *lt*, rather than the two adjacent ones starting from *x* and *lt*. As for "disturbance," Wachsmuth and Springer's own interpretation

of *Dendrocrinus* and *Poteriocrinus* makes *rt* descend into the cup with no disturbance, but with the development of a new plate, which both in *Poteriocrinus* and *Cyathocrinus* occupies the position formerly occupied by *rt* and by the proximal median tube-plate of *Merocrinus* and *Iocrinus*. A similar process is shown by their drawings to have taken place on the left, though to a less extent. This "disturbance" is a mere bogey: if *x* did sink, it did not slide; it gradually grew downwards at its lower margin, as the basal and radianal grew upwards to meet it, and as it gradually became resorbed at its upper margin; and the adjacent plates were not disturbed, but all sank along with it, as has been shown. If "disturbance" is not all nonsense, then take a tube fixed at one end and "composed of five longitudinal rows of hexangular [*sic*] pieces, alternating in adjoining rows," and intercalate a sixth row. Try with bits of card. You cannot do it without "disturbance"; one at least of the adjoining rows must be raised or lowered. So that this "introduction of a new row of plates" effects the very change that it was designed to avoid.

These rather polemical arguments are introduced to show that Wachsmuth and Springer have neither disproved my hypothesis nor proved their own. What they have done is to show that the facts at present known do admit of more than one interpretation from the ordinarily accepted morphological standpoint. They might have adduced the additional argument that the gap between the supposed position of *x* above the radials and its position between the radials, almost coincided with the gap between Monocyclic and Dicyclic Inadunata. *Merocrinus* is the only Dicyclic genus of the former group; *Belemnocrinus* and *Missouricrinus*, the only Monocyclic genera of the latter. This of itself is enough to justify a cry for further facts.

The third conclusion of my 1890 paper is one that I have long since (1892) admitted to be non-proven. Nevertheless our authors deem it worth while to devote considerable space to a discussion of it, in the course of which they evince as much ignorance of their own writings as of mine, and a strange disregard of facts. The conclusion was "that *x* originated as a plate morphologically corresponding to an ordinary brachial," and it was suggested that the median row of tube-plates, or mid-rib, of the anal tube of *Merocrinus* and *Iocrinus*, represented a series of modified brachials (IIBr), up which the ventral perisome around the anus had extended.

The rejection of this hypothesis does not involve the denial of the previous conclusion as to the place of origin of the proximal tube-plate, though my critics persist in confusing the two. Their only serious argument is the impossibility of reconciling my hypothesis with the (alleged) homology between the mid-rib of the *Iocrinus* tube and the median anal series of Camerata, an argument sufficiently discounted by the impossibility of proving the homology in question.

On the other hand, various facts concerning the anal tube of Inadunata, some of which have come to my knowledge since I first propounded this hypothesis, warn us not to reject it too

hastily. They are as follows:—The tube is closely connected with the right posterior ray, it being up this side that the rectum passes; in *Iocrinus*, *Merocrinus*, and *Castocrinus*, and, to a less extent, in *Heterocrinus*, *Ectenocrinus*, and *Ohiocrinus*, the proximal plate of the median row rests on the right posterior superradial;[1] in *Iocrinus* the articulation between the superradial and this proximal plate differs from that between the superradial and the first brachial only in size;[2] in many early Inadunata the

[1] Since I utilized this fact as an argument, Wachsmuth and Springer have persisted in denying its truth and have stigmatized my diagrams as incorrect. If it was not true, why did they in "Revision of Palæocrinoidea," iii, pp. 206, 207, write thus:—In *Ectenocrinus* the r. post. "radial . . . . upon its upper side supports the brachials, giving off laterally a small ventral tube"; and, in *Heterocrinus* "the smaller radial is transversely pentagonal, and resembles in outline a bifurcated plate [*axillare*]. It supports upon its sloping right side the brachials; toward the left the ventral sac as in *Iocrinus*."? My diagram of *Heterocrinus* was, as stated at the time, taken from the drawing of *H. bellevillensis* by W. R. Billings, whose description is in complete accordance with the foregoing remarks, as pointed out in "Crinoidea of Gotland, I" (1893), p. 72. I have no hesitation in saying that this mode of description is absolutely correct. I have most courteously been given the opportunity of studying a very large series of specimens of *Heterocrinus* and allied genera from the Trenton Limestone in the Walcott collection at the Museum of Comparative Zoology, Harvard. In these *x* is invariably closely attached to r. post. Rs, although in many specimens it is clearly separated from l. post. R. In several the RR have become separated from one another, probably by pressure, and, when the posterior interradius is the one exposed, it is always seen that the line of division passes to the left and not to the right of *x*. This proves (so far as the consistent testimony of hundreds is to be held as proof) that the union between *x* and r. post. R is much stronger than that between *x* and l. post. R, even though the geometrical relations of the two radials to *x* may occasionally be similar. But *x* rarely does lie

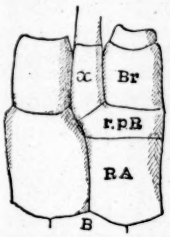


FIG. 17.—Anal area of type-specimen of *Heterocrinus isodactylus* in Dyer Coll. at Mus. Comp. Zool., Harvard. Said by S. A. Miller to be syn. of *Ohiocrinus compactus*. This shows far closer connection of *x* with r. post. Rs than with l. post. R. (Enlarged.)

symmetrically to the adjacent radii: it is nearly always towards the right of the interradius; its two sides abutting on r. post. Rs and its IBr_1 are straight and meet almost at a right angle, whereas the side or sides abutting on l. post. R and its IBr_1 generally form a continuous curve rather than an angle; in all cases where there is any swelling of the radials, or any development of axial folds, then *x* is clearly connected with the swelling or fold of r. post. R, and in no way with that of l. post. R (Fig. 17). These statements, which may be verified by anybody, refer specially to the Heterocrinidæ, though many of them hold good for species not in that family.

[2] Wachsmuth and Springer again criticize my application of the term 'axillary' to the r. post. Rs of *Iocrinus*. They take no notice of my explanation ("Brit. Foss. Crin. III," Ann. Mag. Nat. Hist. [6], vi, Sept. 1890, p. 228). But there is more to be said for the application than I was then aware of. The accompanying drawings (Fig. 18) were made from specimens of *Iocrinus subcrassus* in the Mus. Comp. Zool. Harvard. I shows the interradial, superradial, and first brachial of the r. post. radius from inside; the anal tube is broken off, but its ventral groove is seen passing down the Rs and coalescing in the Ri with the ventral groove of the arm. II shows the articular surface of Ri with the dorsal ligament-fossa, the two muscle-plates, and the already dividing ventral groove. III and IV show the two upper articular

plates of the median series are strangely like arm-plates, both in external view and in section, as I have figured in *Pisocrinus* and *Calceocrinus* ("Crinoidea of Gotland"); in *Iocrinus* the median series has an internal groove, similar to the ventral groove of the right posterior arm, with which it coalesces in the superradial; this evidence, as well as the external folds of the plates, shows that the median anal series of this and similar early forms was innervated from the axial cord of the right posterior ray.

These facts form an accumulation of evidence pointing in one direction. Is there any *a priori* reason why the suggestion should be rejected? It is not so very startling. I have not maintained that an arm-branch with its covering-plates and other organs was metamorphosed into an anal tube. I have not even suggested a change of essential function for the brachials themselves. The function of brachials is to afford a flexible support to extra-calycinal extensions of calycinal organs, and this is the function of the mid-rib of the anal tube in *Iocrinus*. Why, I have asked, may not the tegminal perisome become extended along an arm-branch in the region adjoining the anus, just as we know that it is extended in other regions? We know that arms may become partly incorporated in the cup by a web-like extension of tegminal plates

surfaces of R*s*, III on the left, bearing the median proximal anal, IV on the right, bearing the first brachial; in each are to be seen the dorsal ligament-fossa, two muscle-plates and fossæ, and a wide ventral groove merging into a narrow tongue where the axial cord lay. These facts prove: (1) that r. post. R*s* was united to the proximal anal in the same way as to $1Br_1$; (2) that the anal mid-rib was innervated by the same axial cord as was the r. post. arm. From which we infer (1) that the proximal anal in *Iocrinus* actually is "a plate corresponding to an ordinary brachial" in structure; (2) that the mid-rib may have arisen as a branch of r. post. arm; (3) that, in any case, the application of the term axillare to r. post. R*s* in *Iocrinus* is warranted by something more than the "angularity which occurs upon the upper face," "the form of the plates succeeding it to the left, and the slanting position of the posterior arm to the right" (W. & Sp., p. 129).

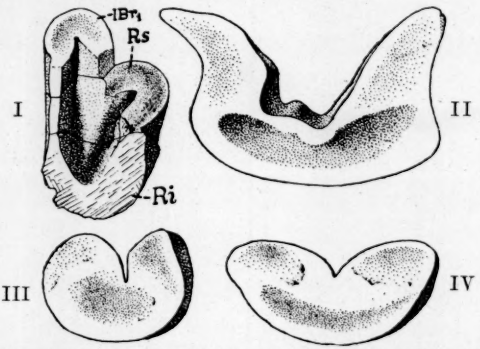


FIG. 18.—Ossicles of *Iocrinus*, probably *I. subcrassus*, in Mus. Comp. Zool. Harvard. I. Right posterior interradial (R*i*) and superradial (R*s*) and first primibrach (IBr_1) seen from interior of cup. II. Upper articular surface of R*i*; groove on left goes to *x*, that on right to brachials. III. Left upper articular surface of R*s*, supporting *x*. IV. Right upper articular surface of R*s*, supporting $1Br_1$. (Enlarged.)

between them. In *Uintacrinus* some pinnules (which are only lesser arm-branches) are so modified by this incorporation that they have lost their original brachial character almost to their tips. *Calamocrinus* belongs to a family in which there is as a rule no anal tube of any consequence; but in this genus the tube is 20 mm. long and 5 mm. in diameter half-way up, and, according to Alexander Agassiz, it was "for more than half its length rigidly soldered to the edge of the adjacent arm and pinnule joints."[1] The

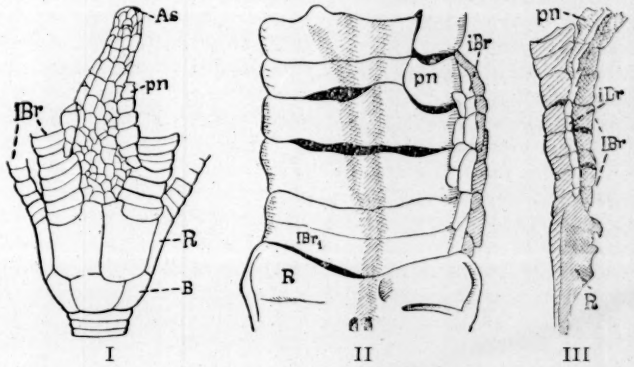


FIG. 19.—*Calamocrinus Diomedeæ*.

I. Posterior view of cup, showing attachment of anal tube to pinnules and proximal brachials. (× ⅔, from pl. iii, fig. 3.)

II. Radial and proximal brachials seen from inside of cup, showing attachment of interbrachials. (Much enlarged, and combined from pl. iii, fig. 6, and pl. xx, fig. 2.)

III. A similar portion seen from the side, showing the interbrachials (unshaded) attached to the brachials and pinnulars (shaded). (Enlarged, from pl. iii, fig. 5.)

All after Al. Agassiz. *As*, anus; B, basal; IBr, primibrachs; iBr, interbrachials; *pn*, pinnulars.

food-grooves of the pinnules involved enter the tegmen at some distance from the bases of the pinnules, and thence pass to the mouth. Thus half the pinnule supports a food-groove with associated organs, while the other half supports an anal tube (Fig. 19). We have only to suppose the attachment of the tube to the first pinnule of the right posterior arm for a greater distance, and we should see a state of things almost precisely similar to that which, on my hypothesis, was the case in *Iocrinus*.

There is, then, no difficulty, morphological or physiological, in my hypothesis. But it will not be proved until there be found an ancestor of the Heterocrinidæ with the mid-rib of the anal tube free at its distal end and grooved. To such a condition that otherwise incomprehensible structure, the anal appendage of *Thaumatocrinus*, may be a partial reversion.[2]

[1] "*Calamocrinus Diomedeæ*": Mem. Mus. Comp. Zool. Harvard, vol. xvii, No. 2, Jan. 1892; see p. 36. The adjoining figures are adapted from this marvellously detailed work.

[2] P. H. Carpenter, "On a new Crinoid from the Southern Sea": Phil. Trans., 1883, pt. iii, pp. 919-933, pl. lxxi; see pp. 925, 930.

A more serious difficulty than any mentioned by Wachsmuth and Springer seems to me to lie in the existence of an anal between the posterior radials of *Hybocystis* and *Hybocrinus* (Fig. 6). These primitive genera undoubtedly suggest the view that both anal *x* and the radianal were original elements of the dorsal cup. I used once to think that some such view had been maintained by Wachsmuth and Springer,[1] but they have stoutly disowned the imputation.[2] On that view, both anal tube and arms would be outgrowths of the thecal wall, containing extensions of internal organs, and composed of a dorsal or outer series of supporting plates and a ventral or inner series of protecting plates. Thus the mid-rib of the anal tube would still be in a general way homologous (or more definitely, 'serially homoplastic') with the brachials; and *x* would be the serial homologue of the plate that we have been speaking of as the right posterior superradial, which would really be a modified brachial. The difficulties of this interpretation are obvious, but perhaps not insuperable.

On both my hypotheses, then, *x* corresponds more or less to a brachial, and is, as are the other anals of Inadunata, a constant element, the evolution and gradual disappearance of which may be traced. On the last hypothesis of Wachsmuth and Springer, *x* differs from all the other anals in being a supplementary plate, suddenly introduced, and almost as suddenly disappearing. In the absence of definite proof, each of my readers must decide which of the three views he will adopt as a working hypothesis. F. A. Bather.

(*To be continued.*)

II.—The Geology of the Country around Eastbourne. By Clement Reid, F.L.S., F.G.S. Geological Survey Memoir. 8vo; pp. iv, 15. (London, 1898. Price 6*d*.)

Among the most useful of the Geological Survey publications are the explanations to accompany the various sheets of the maps. In this respect the Irish Survey has set the best example, and has issued a complete series of Explanatory Memoirs. Much yet remains to be done in England and Wales and in Scotland to bring the work similarly up to date.

Now that the Survey in England and Wales is issuing its sheets on the new series of the Ordnance Survey Map the opportunity is taken of publishing short Memoirs, of which those by Mr. Reid on Bognor and Bournemouth have lately been noticed. We are now able to call attention to the issue of another on the Geology of Eastbourne, and this deals with the Cretaceous rocks from the Weald Clay to the Upper Chalk, with the Woolwich Beds and London Clay of Newhaven and Seaford, and with sundry superficial deposits. Illustrations of the characteristic fossils are given, and there are notes on disturbances and surface-erosion, on water-supply and economics.

[1] "Brit. Foss. Crin. II": Ann. Mag. Nat. Hist. (6), vol. v (April, 1890); see pp. 322, 324.

[2] "The Perisomic Plates": Proc. Acad. Nat. Sci. Philadelphia, vol. for 1890, Feb. 1891; see pp. 389, 390.

REPORTS AND PROCEEDINGS.

Geological Society of London.

I.—November 23, 1898.—W. Whitaker, B.A., F.R.S., President, in the Chair.

Sir A. Geikie drew attention to some specimens on the table which had been collected by the Geological Survey from the Silurian rocks of County Tipperary. These contained impressions that bore a close resemblance to flattened and drawn-out graptolites, and others that might be taken for mollusca or phyllopoda enlarged by cleavage. It seemed to him, however, extremely doubtful whether these forms were truly of organic origin. They were exhibited in the hope that the palæontologists in the Society might be able to throw some light upon them from the zoological side.

Dr. G. J. Hinde exhibited and commented on specimens of Devonian rocks sent by Prof. Edgeworth David and Mr. Pittman from the railway section at Tamworth, New South Wales, which had been received since their paper was read. The specimens included radiolarian shales and submarine tuffs containing impressions of *Lepidodendron australe*; similar shales with bands of tuff regularly interstratified; radiolarian chert irregularly broken up and intermingled with tuff; and hard shale with numerous radiolaria weathered out on the surface. Microscopic sections prepared from these rocks were also exhibited.

The following communications were read:—

1. "Note on a Conglomerate near Melmerby (Cumberland)." By J. E. Marr, Esq., M.A., F.R.S., F.G.S.

In this paper the author describes the occurrence of a conglomeratic deposit which shows indubitable effects of earth-movement, not only on the included pebbles, but also on the surface of one of the deposits. The rocks are coloured as basement Carboniferous rocks on the Geological Survey map. The Skiddaw Slates are succeeded by about 30 feet of a roughly stratified conglomerate, followed by 20 to 30 feet of rock with small pebbles, and that by a second coarse conglomerate. The pebbles possess the outward form of glacial boulders, but many of them are slickensided, fractured, faulted, and indented. The striæ are often curved, parallel, and covered by mineral deposit; the grains of the matrix are embedded in the grooves, while slickensiding often occurs beneath the surface of the pebbles and the striæ are seen to begin or end at a fault-plane. The surface of rock beneath the upper conglomerate was found to be slickensided. The way in which the surfaces of some of the pebbles have been squeezed off suggests the possibility that their angular shape may be partially or wholly due to earth-movement.

2. "Geology of the Great Central Railway (New Extension to London of the Manchester, Sheffield, and Lincolnshire Railway): Rugby to Catesby." By Beeby Thompson, Esq., F.G.S., F.C.S.

In this paper the portion of the line, 10 miles in length, from

Catesby to Rugby is described; as the ground falls while the strata rise in this direction, quite low beds in the Lower Lias are met with near Rugby. The lowest zone exposed is that of *Ammonites semicostatus*, in the lower part of which, and in Boulder-clay derived from it, *A. Turneri* has been found. The next succeeding zone, that of *A. obtusus*, although for the most part barren, yielded the characteristic fossils at its base. The *oxynotus*-zone is well developed and well displayed, besides being richly fossiliferous. The zone of *A. raricostatus* merges into that of *A. oxynotus* below and that of *A. armatus* above, and is not more than three or four feet thick. The *armatus*-zone, beds between that and the *Jamesoni*-zone, and the *Jamesoni*-zone itself follow; the middle beds of the latter being rich in *Rhynchonella* and *A. pettus*, the name of this ammonite is attached to the zone bearing them. The *Ibex*-zone occurs east of Flecknoe, covered by rocks yielding *A. Henleyi*; and the highest beds of this cutting appear to belong to the *capricornus*-zone. Lists of the characteristic fossils of each zone are given, followed by a complete list of all those found in the Lower and Middle Lias of the cuttings, with a statement of their distribution.

The glacial deposits are described under the following headings: Blue or local Boulder-clay, brown and grey contorted Boulder-clay, Chalky Lower Boulder-clay, (Mid-Glacial) sands and gravels, and red Upper Boulder-clay.

The paper is accompanied by a measured section along the railway.

3. "On the Remains of *Amia* from Oligocene Strata in the Isle of Wight." By E. T. Newton, Esq., F.R.S., F.G.S.

The specimens described in this communication were found by Mr. Clement Reid in the Bembridge Marls of Hamstead, and by Mr. Colenutt in the Bembridge Beds and in the Osborne Series of King's Quay, near Ryde. After a reference to species described in America and referred to the genera *Amia*, *Protamia*, *Hypamia*, and *Pappichthys*, the author proceeds to the description of the specimens in question, referring them all to the genus *Amia*. The specimens include the following bones: vertebræ, maxillæ with the supplementary bones, premaxillæ, bones of the skull, dentary bones, a parasphenoid, a clavicle, scales, and teeth. They are referred to two new species of the genus *Amia*. The paper concludes with a table of all the species hitherto recorded from America and Europe.

II.—December 7, 1898.—W. Whitaker, B.A., F.R.S., President, in the Chair. The following communications were read:—

1. "The Geological Structure of the Southern Malverns and of the adjacent District to the West." By Prof. T. T. Groom, M.A., D.Sc., F.G.S.

The Raggedstone and Midsummer Hills, consisting essentially of massive gneissic and schistose rocks, are traversed by a curved depression which marks a line of profound dislocation, probably of the nature of a thrust-plane. This appears to dip towards the east, though with a relatively small hade. Along this depression occur strips of Cambrian and Silurian strata embedded in the Archæan

massif, and indicating the presence of a deep and narrow dislocated synclinal fold. In places, the foliation of the schists shows a marked relation to the direction of this line, indicating in all probability a production of schists from the old material in post-Lower Palæozoic times.

The western boundary of the Archæan massif is everywhere a fault, apparently a thrust-plane, but with a small hade. The direction of this plane is in close relation with the axis of the overfold into which the Cambrian rocks are thrown to the west of these hills.

The western boundary of Chase End Hill is likewise a fault, which is probably a thrust-plane with a tolerably low dip towards the eastern side. The thrust here also appears to have been accompanied by a secondary production of schists from the old gneissic series; and the Cambrian strata are overthrown in the vicinity of the fault.

There is no evidence for the overlap of the Cambrian Series supposed by Holl, the circumstance that the various zones of the Cambrian Series strike up against the Archæan axis being due to faulting.

The Cambrian is represented by the following series:—

4. Grey Shales.
   - Upper Grey Shales.
   - Coal Hill Igneous Band.
   - Lower Grey Shales.
   - Middle Igneous Band.

3. Black Shales.
   - Upper Black Shales.
   - Upper White-leaved-Oak Igneous Band.
   - Lower Black Shales.
   - Lower White-leaved-Oak Igneous Band.

2. Hollybush Sandstone.
1. Hollybush Quartzite and Conglomerate.

Fossils are abundant in certain zones of each of the four subdivisions of the series.

The Grey Shales rest conformably on the Black Shales, but the mutual relations of the remaining subdivisions can be decided only by inference, the junctions being apparently everywhere faults. The junction between the Cambrian and Archæan is likewise a fault.

All four divisions of the Cambrian Series are invaded by small igneous bosses, laccolites, and intercalated sheets of diabase and andesitic basalt. These igneous rocks do not penetrate the May Hill Series.

The May Hill Beds seem to rest with apparent conformity upon the Grey Shales, and do not transgress across the various Cambrian zones on to the Archæan in the manner hitherto supposed, the presumed outliers being small patches faulted into the Cambrian.

The structure of the district is to be explained on the supposition that we are dealing with the western margin of an old mountain-chain overfolded towards the west; the eastern portion of this range lies faulted down and buried beneath the Permian and Mesozoic of the Vale of Gloucester. All the characteristics of a folded chain are present, namely, the profound folds, overfolds, thrust-planes, and transverse faults; and a typical Austönungs-zone is seen to the west.

2. "The Permian Conglomerates of the Lower Severn Basin." By W. Wickham King, Esq., F.G.S.

The rocks thus described are the calcareous conglomerates included in the Middle Permian of the Shropshire type, and exposed north of the Abberley and Lickey Hills. Three calcareous horizons occur, interstratified in sandstones or marls and surmounted by the Permian breccia. It was the opinion of Ramsay and others that the materials of the calcareous horizons and of the Permian breccia had been brought from the Welsh border; but Buckland and Jukes, among others, claimed a southern derivation for those of the Permian breccia, from local hill-ranges to the south. The latter view accords with the fact that the pebbles composing these calcareous horizons, and also the broken fragments constituting the Permian breccias north of the Abberley and Lickey Hills, are coarser in the south-easterly direction, and gradually become finer to the north-west.

The fragments embedded in the Middle Permian calcareous bands near the Lickey are chiefly of Archæan rocks, but in all the other districts described there are very few rock-fragments older than Woolhope Limestone. On the other hand, pebbles of dolomitic Wenlock and Carboniferous Limestones are abundant, while Aymestry Limestone, Old Red, Carboniferous, and Lower Permian Sandstones occur in greater or less abundance; and all these rocks, except the Carboniferous Limestone, may be seen *in sitû* near at hand to the south. A summary of work done in the Halesowen Coal-measure conglomerates and in the Permian breccia north of the Abberley and Lickey Hills is given, to bring out one of the lines of argument adopted.

(1) Ridges near the Lickey were denuded down to the Archæan rocks in Upper Carboniferous time; therefore, as might have been expected, both the adjacent Upper Carboniferous conglomerate and the Middle Permian calcareous cornstones are composed of such fragments of Archæan rocks as are to be found *in sitû* there, or at Nuneaton; and the Upper Carboniferous conglomerate is also largely composed of Palæozoic rocks identical with those *in sitû* on the flanks of the Lickey.

(2) The Middle Permian calcareous conglomerates of the other districts described are for the most part made up of fragments not older than the Woolhope Limestone, which were presumably derived by denudation from ridges which had become more extensive.

(3) The Lickey ridges having been denuded to the Archæan rocks and the more extended area to the Woolhope Limestone, the later Permian breccias are composed of Archæan fragments near the Lickey, but of rocks not newer than the Woolhope Limestone in the other districts north of the Abberley and Lickey Hills.

The author has for several years called the ancient ridges from which these materials were derived the 'Mercian Highlands,' and claims that the Palæozoic and Archæan rocks composing the stumps of these highlands lie almost entirely buried under the Trias of the Midlands south and east of the S.E. Shropshire and S. Staffordshire regions.